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SEVENTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

[From our own Correspondents.]

SATURDAY, SEPT. 9.

THE General Committee assembled at one o'clock, in the Library of the Athenæum, the Marquis of Northampton, President for the last year, in the chair. The illness of Dr Dalton, the elevation of the Rev. E. G. Stanley to the see of Norwich, and other circumstances, rendered some changes necessary in the officers. The Rev. W. Whewell was added to the list of Vice Presidents; and Professor Traill, Mr. W. W. Currie, and Mr. J. N. Walker were appointed Secretaries to the Meeting. The appointment of Mr. W. Earle, mayor of Liverpool, as Chairman of the Local Committee, was confirmed. Mr. Yates read a Report of the proceedings of the Council. It contained several recommendations, which will be noticed as they come under discussion; and it announced that Her Majesty's government, at the request of the Council, had consented to repeal the duty on the importation of philosophical instruments, and also to extend the Ordnance Survey to Scotland. It was also stated that the Council had directed its attention to the establishment of a law of universal copyright, and had entered into communication with M. Guizot, Minister of Public Instruction in France, and Mr. Serjeant Talfourd. A plan for effecting this desirable object had been forwarded to M. Guizot, but his answer had not yet been received.

It was then recommended that the next place of meeting, and the election of officers, should be determined at a meeting distinct from that in which the financial business was brought under consideration; the former custom of considering these objects in the last hurried meeting having led to great inconvenience, and especially prevented the Committee of Recommendations from fully considering the grants they had to propose to the General Committee, and the latter body from deliberating on the recommendations. After some debate, it was resolved that a meeting should be held to determine the next place for the assembling of the Association, and also to choose officers, at three o'clock on Thursday the 14th instant; and a meeting for sanctioning the grants of the Committee of Recommendations, on Saturday the 16th at one o'clock; and it was recommended that the Sectional Committee should transmit their several propositions to the Committee of Recommendations, if possible, before twelve o'clock on Friday the 15th instant. A discussion then arose, respecting some proposed changes in the constitution of the General Committee. The Council submitted that the admissions to the body should be thus classed:—

1. Presidents, Vice Presidents, and office bearers of the Association, and contributors to its Reports.
2. Presidents, Vice Presidents, and office bearers of all chartered Philosophical Societies publishing Transactions, and contributors to those Transactions. The same privilege to be extended to the following unchartered societies:—The Literary and Philosophical Society of Manchester; The Natural History Society of Newcastle; The Wernerian Society of Edinburgh; The Royal Geological Society of Cornwall.
3. Three officers or delegates from each of the preceding Societies, and also from The Bristol Institution, and The Philosophical Society of York.

It was moved by Mr. James Heywood, and seconded by Colonel Sykes, that the names of the Statistical Societies of London and Manchester be added to the list No. 2. Considerable discussion ensued; after which Professor Whewell proposed that they should be added to list No. 3, which passed unanimously.

Professor Apjohn then moved that the name of the Geological Society of Dublin be added to list No. 2. This was seconded by Mr. Murchison, and passed without opposition.

The Council was empowered to nominate eminent individuals as members of the General Committee for this meeting.

The following Members of the General Committee were chosen Members of the Committee of Recommendations, in addition to the office bearers of the Association:—F. Baily, Esq., Sir D. Brewster, Professor Christie, Mr. Greenough, Col. Sykes, Professor Peacock, J. W. Lubbock, Esq., Dr. Roget, Dr. Richardson, Professor Whewell, Professor Lloyd, and Sir W. Hamilton.

Sectional officers and committees were then appointed, and the Committee adjourned.

The Presidents and Vice Presidents of the Association and the Sections dined with the Mayor at the Town Hall, and there was a *soirée* for the Members generally at nine o'clock in the evening.

The following instructions were issued to the Secretaries of Sections, the Officers and Members of Committees:—

Committee meetings to be held every day of the week, from ten to eleven o'clock.

The proceedings of the Committee of each Section to be commenced by the Secretary reading the Resolutions adopted last year by the Committee of the same Section. It is then to be inquired whether the Reports and Researches intrusted to particular persons or committees, at the late or preceding meetings, are executed, and in what degree the desiderata in science are supplied by communications to the Sections.

The Committees of Sections are requested to communicate to the Committee of Recommendations any suggestions which they may think useful for the advancement of science. They are specially requested to review the recommendations contained in the 4th and 5th volumes of Reports, and the communications made at the meeting, for the purpose of selecting definite points of research to which individual or combined exertion may be usefully directed; and branches of knowledge, on the state and progress of which reports are wanted; to name individuals or committees for the execution of such researches or reports; and to state whether, and to what degree, these objects may be usefully advanced by the appropriation of the funds of the Association, or the application to government, philosophical institutions, or local authorities.

In case of the appointment of committees for special objects of science, it is recommended that all Members and a Secretary should be named—such committees having power to add to their number, persons whose assistance they may desire.

Grants of pecuniary aid from the funds of the Association expire at this meeting, unless a continuation of the grants be applied for by the Sectional Committee, and ordered by the General Committee.

Copies of the minutes of the proceedings of the Committee are to be forwarded as soon as possible, the same morning, to the Assistant General Secretary, Royal Institution.

SECTIONS.—The Sections will open at eleven, A.M., and close at three, P.M. There will be no meeting on Saturday.

The communications to be taken in a settled order, as previously arranged by the Secretaries, and made public by notices in the vestibules of the Town Hall and Mechanic's Institute, as well as on the doors of each Section-room. Reports and papers read, or copies of them, are to be forwarded to the Assistant General Secretary.

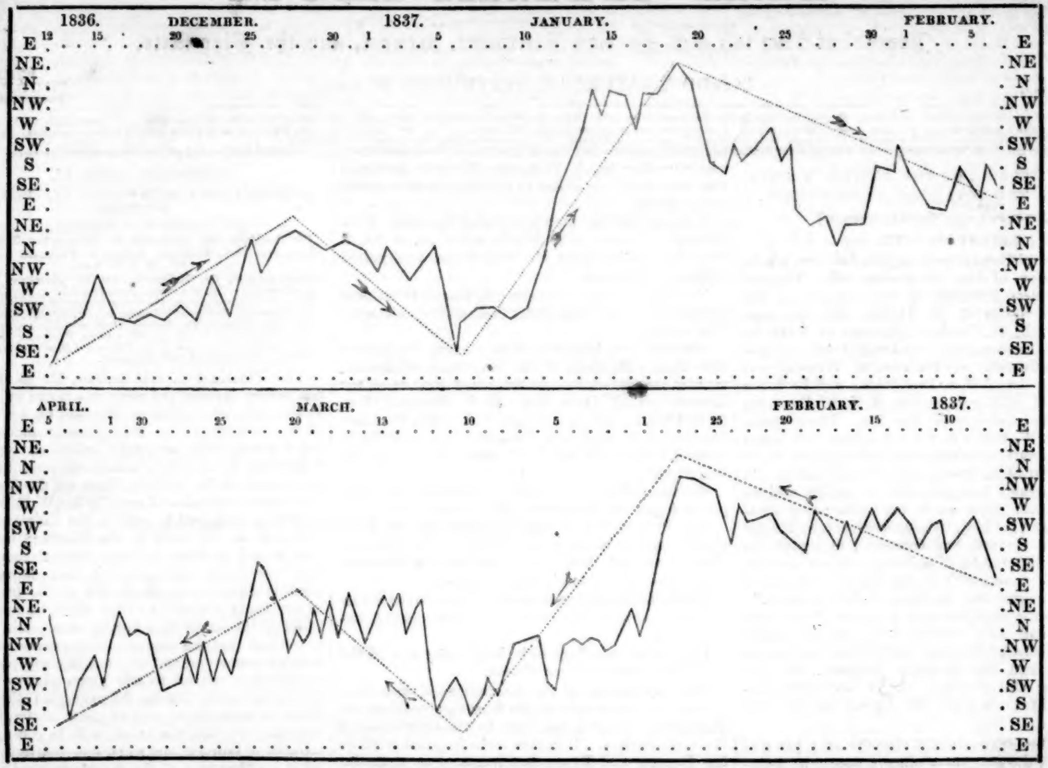
MONDAY, SEPT. 11.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President—Sir D. BREWSTER.
Vice Presidents—Mr. LUBBOCK, Mr. BAILY, Rev. G. PEACOCK.
Secretaries—Rev. Professor POWELL, Professor STEVELLY, Mr. W. S. HARRIS.

Committee—Rev. Dr. Robinson, Sir W. Hamilton, Professor Christie, Rev. W. Whewell, Professor Lloyd, Professor De La Rive, Professor Plateau, Professor Henry (of New Jersey Coll.), Dr. Faraday, Professor Wheatstone, Dr. Ritchie, Major Sabine, Rev. Dr. Hincks, Mr. Russell, Professor Moll, Captain Ross, Mr. R. W. Fox, Professor M'Cullagh, Professor Challis, Mr. J. H. Abram, Professor J. S. Bagge (of Stockholm), Rev. Mr. Dawes, Mr. Dollond, Mr. Hopkins.

The President of the Section, Sir D. Brewster, not having arrived, the chair was taken by the Rev. G. Peacock, who addressed the Section, and informed the members of the arrangements made for the day's proceedings, and then called on one of the Secretaries to read a communication entitled 'Suggestions as to the probable causes of the Aerial Currents in the Temperate Zones,' by Mr. W. R. Birt. The author endeavoured to explain the variations in the direction of the wind by the following hypothesis. The heated air from the inter-tropical regions flows over towards the poles, giving rise to currents in every possible direction on either side of the tropics, and as the sun is vertical to a spot which traverses a parallel of the torrid zone during twenty-four hours, it is evident that the currents thus generated are extremely numerous, and situated in every possible direction with respect to any given place within the temperate zones, London for example: the paths of these currents being arcs of great circles, each, except the one from the south, will be a tangent to a parallel of latitude, and when it arrives at that parallel, it is either an easterly or westerly wind, according to the direction in which it arrives. Upon inspecting a terrestrial globe the reader will obtain a tolerable idea of the form and extent of the paths which these currents traverse in their progress over the temperate zones, and he will perceive that some are much longer than others, in consequence of which they will arrive at the place of observation much later. The author supposes that these currents are found about the 30th parallel of north latitude for the northern temperate zone, and finds that those which originate between two hours east and two hours west longitude while the sun is traversing that portion of the torrid zone, and which are directed towards England, will arrive at London as S.E., S. and S.W. winds, the S.E. and S. appearing first, and the S.W. succeeding them: he then traces the variation of the wind in the order S.W., W., N.W., N. and N.E. as the sun becomes vertical to portions of the torrid zone situated more and more westerly; the westerly winds originating between four and five hours west long. where their direction at the 30th parallel is nearly S.W.; the north-westerly are produced at the 10th meridian west of Greenwich, the northerly at 180° of longitude, and the north-easterly at the 10th meridian east of Greenwich; he also finds that the easterly winds originate between four and five hours each. This view of the formation of currents on the equatorial boundaries of the temperate zones Mr. Birt considered sufficient to explain the variation of the wind in the order above mentioned which he terms direct, and which is indicated in the graphical delineation of the course of the winds by which the paper was accompanied, (a sketch of which will be found in the next page,) by the ascending dotted lines; he also accounts for the greater prevalence of the S.W. winds in consequence of the small spaces which they traverse compared with the extent of the paths of the northerly currents. On inspecting the diagram it will be seen that there are alternate ascending and descending dotted lines, and that the general tendency of the wind was to vary in the order S.E., S., W., N. and N.E. during the ascending periods, and N.E., N., W., S. and S.E. during the descending, which are termed retrograde. In order to account for the retrograde movements, Mr. Birt stated that, "England from its position in



the northern temperate zone is subjected to the influence of those currents that proceed from the pole towards the equator to supply the place of the air which ascends by the heating power of the sun; these currents arrive at London from all points between W. and E. towards N. in the order W., N., E., and may concur in their arrival with the N.W., N. and N.E. equatorial currents, or the S.E., S. and S.W., producing a diversity of phænomena according as they are situated, west or east of them; if, for instance," he observed, "a N.W. polar current arrives with a N. equatorial, the resultant wind is N.N.W., and if the equatorials have steadily proceeded from S.E. to N., in consequence of the position of the two kinds of currents a regression will take place, which will be greater the farther the winds are from each other." Mr. Birt considered that this state of the currents will materially influence the remaining winds, and, when combined with the meeting of differently posited equatorial currents, a permanent regression of a greater or less magnitude may be induced, which will give place to a direct order upon the polar currents preceding the equatorial in their arrival. The paper closed with a remark that the diagram suggests the idea that the revolution of the equatorial and polar currents are extremely regular, and that the antecedence and consequence of the polar, relative to the equatorial, are subject to laws capable of being ascertained by careful observation. During the first half of the period tabulated, namely, from December 12th, 1836, to February 7th, 1837, there were two alternations of the equatorial and polar, and during the latter half from February 7th, to April 4th, the same alternations occurred in a reverse order.

The President suggested that as the author was not present, it would, perhaps, be as well not to have any discussion on the paper. The Secretary then read a communication from Col. Gold, late of the Royal Artillery, 'On the possibility of effecting Telegraphic or Signal Communications during Foggy Weather and by Night, in all seasons.' The author prefaced his remarks by referring to the frequent interruptions in transmitting information by telegraph during dense atmospheric weather, or by night. These interruptions are not to be overcome by any improvement in the ordinary mode of *visible* signals, and, therefore, it is most desirable, if possible, to find a substitute for that mode, which shall be entirely independent of atmospheric influence. Sound seems, on a first consideration of this subject, to furnish the readiest and most simple means of distant communication; and this is to some extent already adopted on shipboard during foggy weather, as in the case of *caution sounds* of guns, drums, bells, &c. to warn others from too near an approach; and, doubtless, the numerical repetitions of gun or any other sounds, might be applied to corresponding tables of words and sentences on every subject desirable to have reported by telegraphic means. A serious objection, however, to the use of this method on shore, is the noise and alarm created by the discharges. It could not, however, be fairly urged against it, that the messages sent would be subject to publicity or exposure, any more than the usual telegraphic signals, for, without the key, each would be equally unintelligible. As to the relative rapidity of these two methods, flashes of light are seen at any distance almost instantaneously; while, although sound travels at the rate of 1142 feet per second, a very reasonable messenger's pace, yet its adoption is prevented by reasons which have been before stated. Any attempt to use projectiles, propelled by gunpowder, would be dangerous, yet this the author stated was the most rapid agent hitherto in the power of man to employ: but a velocity of 1200 or 1600 feet per second is, indeed, produced at the moment of discharge, but in consequence of another action of the atmosphere—viz. its resistance to the motion of bodies passing through it,—this soon lessens to an extent sufficient to render the method unavailing, even were the danger of throwing hollow shot or shells, with enclosed written communications, less than they would be found in practice. He conceived, then, that an agent of much greater power was to be had in ordinary Electricity—an agent every way preferable, if it can be applied; which, he conceived, might readily be effected, by laying strong and appropriate wires, for many miles, in stone troughs, parallel to the lines of our railways, and with proper points of what the author called electric renovation, or what may be termed relays. These, he conceived, might be so employed as to afford the desired communications, public, commercial, and private, at a moderate expense, compared with the great objects attainable at all times and seasons. The author wished to refer, with deference to the accomplished practical electrician, Mr. Cross, the feasibility of this suggestion, admitting that it was open to difficulties, as to the security of the passing wire, by a non-conducting envelope of gum lac, Indian rubber, or other proper substance; and he conceived the extensive experience of that gentleman to be indispensable, for ascertaining the degrees of electric power available for such purposes, as well as the length of wire through which the electric fluid might be made to travel, so as to be capable of emitting flame, or decided spark, at the end of its transit. From a repetition of these sparks, under proper arrangements, the signal, he conceives, might be read off from the *key-book* of reference in the usual way, by numerical order. The author then referred to experiments performed on Shooter's Hill, where the discharge was instantaneous when made through a circuit of wire two miles in extent, stating, however, that he himself had no sufficient data on which to ground any calculation. Then assuming as a moderate basis for calculation, a degree of rapidity no greater than that of sound,—suppose 1200 feet, or 400 yards per second, which is the initial velocity of a cannon shot impelled by a full charge of powder, or $4\frac{1}{2}$ seconds per mile; and then allowing a reduction of one-fourth for some slackening or loss of time at the relay stations,—that is, supposing the speed to be only one mile in six seconds, or ten miles a minute—this is 200 miles in 20 minutes, which is a good rate of intercourse, though, perhaps, considerably less than what it would actually be, or than the rate of the common telegraph *when it can work;* but this, as has been before said, is often impossible. The great object being to anticipate all communication by the mail to the utmost possible extent, this object would be attained, even by the low calculation now entered into, thirty times quicker than by the mail, even when conveyed by the rail-

road, at the rate of twenty miles per hour, and this might frequently be of the utmost consequence.

At the conclusion the President stated, that for similar reasons to those which induced the Members of the Sections to refrain from comment upon the last paper, he supposed this would pass without any remarks; which was the less to be regretted, as many valuable experiments were now in progress, with a view to the application of electro-magnetic phænomena to the rapid propagation of signals.

The Secretary next read a communication, forwarded to the Association by N. L. Beamish, Esq., 'On an improved mode of constructing Magnets,' by Mr. Cunningham, of Cork. The author stated, that as a cheap method of making magnets had now become a subject of deep importance and very general interest, chiefly as related to their use in magneto-electrical machines, and having some time since turned his attention to the subject, he had tried steel of various qualities, but could not succeed in making magnets sufficiently powerful for the above purposes. While thus engaged, he met with a communication from Mr. Knight, jun. to the Society for the Encouragement of Arts, published in their 50th volume, describing a new mode of construction, and recommending common blister steel of an open grain and highly carbonized, as the best material for the purpose; and since much hammering is highly injurious, he advised that the steel should be procured of the size exactly suited to the required magnets, which would obviate the necessity of forging, except at the centre of the rod, to give it the horse-shoe form. The author stated, that it would be an obvious improvement upon this method, to give iron the required form previous to its conversion into steel, as this would render any further disturbance unnecessary. It occurred to him, while reflecting upon the very large quantity of carbon in cast iron, that it ought to be admirably adapted to this purpose, and, in order to test the correctness of his opinion, he got three small castings made of the horse-shoe form, each weighing seven ounces; on touching these with a small compound magnet in the usual manner, he was very agreeably surprised to find them absorb and retain the magnetic influence in a degree superior to any steel ones he had ever previously constructed; and stated, that he had no doubt that they would be further improved if beaten red-hot, and very slowly cooled, which would make the metal softer, and the grain more uniform, and they might afterwards be hardened at the poles to produce the maximum effect. He considered this result of much importance, as it will enable us to construct compound magnets for magneto-electrical machines with great facility, and at a very trifling expense, as any number can be cast from one timber pattern.

Mr. Christie stated his regret that the gentleman, whose communication had been just read, had not entered into numerical details, stating as well the power of the steel magnets which he had previously made, as the actual and comparative power of the cast-iron magnets, which he seemed so much to prefer. It must be obvious, that without this information, no scientific person could at all confide in the vague statement, that magnets made in one particular way were superior to those made in another way, for both might be badly made, and the difference might not be of much practical importance. He strongly doubted the value of cast iron as a material for the construction of magnets.—Mr. Holden said, that he had bestowed much time and attention on the construction of magnets. He preferred steel tempered blue, or to spring temper, and was, on the whole, inclined to agree with the last speaker, in doubting the value of the material proposed. He knew that cast iron was capable of receiving strongly the magnetic influence, and bars of cast iron, as long as they retained their upright position, were found to possess polarity in a very high degree; but he doubted whether, if they were removed from their upright positions, they would long retain their polarity to any considerable extent.—Mr. Peacock fully agreed with Mr. Christie, that the information conveyed in this paper was of too vague a character to be much depended on, or to be of much practical benefit: there were, however, so many individuals engaged in these investigations that he did not doubt but that very soon the materials best suited to the construction of magnets would be decided on, if, indeed, that had not been already done.—Mr. Christie stated, that, at the very last meeting of the Association, in Bristol, the Rev. Mr. Scoresby had made a communication respecting the construction of magnets, and the material he preferred was very evenly-tempered steel, of the blue temper, such as watch springs, or ladies' busks. He had also strongly reprobated the leaving any part of a magnet soft, and hardening the rest.—Mr. Stevelly begged leave to ask Mr. Christie, or Professor Lloyd, whether Captain Kater had not, in his extensive experiments, examined cast iron as one of the materials fitted for the construction of magnets; for if so, he, of course, had stated the comparative merits numerically, his object being to determine the best material for the construction of compass-needles.—Mr. Christie replied, he did not remember whether Captain Kater included cast iron or not, in his very extensive and valuable series of experiments; but of one thing he felt certain, that he had not recommended it as the best material.—Mr. Snow Harris observed, that, from many trials, and much experience, he was convinced that hardened steel wire, just as it is to be had in the shops, without any further working it, or putting it into the fire, or altering its temper, was the best material for constructing small needles, intended to retain their magnetism permanently; and this latter consideration was of the utmost consequence when constructing needles for philosophic research—as, for instance, upon the magnetic intensity at various places, since the slightest alteration of power, in that case, would most materially and injuriously affect the result.—Prof. Henry had tried cast iron, and had found that it did not retain its magnetic power so effectually as common steel.

Mr. Lubbock then gave an account of the Discussion of Observations of the Tides, obtained by means of the grant of money placed at his disposal at the last meeting of the Association.

The author stated, that, at the last meeting, he had reported the progress then made in the discussion of the tide observations, and the results he had obtained. It was now his wish to explain the manner in which the last grant of money had been employed. He had engaged Mr. Jones to discuss 13,391 observations of the tides, made in Liverpool during 19 years by Mr. Hutchinson, with reference to a previous transit of the moon, or that which precedes the time of high water by about 48 hours. These observations, he stated, were the property of the Lyceum, and had been with great kindness granted by that Institution for the purpose of this inquiry. He had also engaged Mr. Russell to extend the former discussion of the London Dock observations, by employing all the observations made from the 1st September, 1801, to the 31st of August, 1836, being 24,592 observations. Tables had thence been formed in the same manner as those already submitted to the Section at Bristol; and, although, in some instances, irregularities had been in consequence detected, yet the agreement with the averages of observations conducted for 19 years, amounting to 13,370, had been much closer than the author had anticipated. He had also engaged Mr. Russell to examine carefully the establishment, and the average height of high water, in order to ascertain the fluctuations to which these quantities are subject. He passed a warm eulogium on the zeal displayed by Mr. Jones and Mr. Russell, and the laborious exertions which they had made to render the final results as accurate as the nature of the subject would permit; and he placed upon the table several thick quarto volumes, containing the details of these numerical discussions. He then proceeded to state the results of these discussions, and commenced with the parallax inequality for the interval,—premising, that even minute discrepancies between the results afforded by the London and Liverpool observations were interesting, and deserved illustration. Whatever may be the law of the parallax inequality of the tides, he conceived that we may certainly conclude, that it is proportional to the difference of the parallax from 57′; and on this principle he explained how all the averages employed to give the inequality for various actual parallaxes may be combined. In this manner Mr. Russell had combined all the results afforded by the 13,391 Liverpool observations, as well as those afforded by the 24,592 London observations, so as to produce for each place the inequality in the interval, and height for horizontal parallax 54′. The tables resulting from this combination might therefore be considered as the averages of more than 1,000 observations for Liverpool, and more than 2,000 for London. He then exhibited diagrams, exhibiting to the eye the results of these tables, in which the intervals and heights, as deduced from theory, were marked by red curved lines; while those deduced from the observations at Liverpool were marked by dotted curves; those from observations at London by black curved lines. The curves for the interval, deduced from theory, and from observation at Liverpool, were almost identical; while that from theory, when compared with the curve derived from the observations at London, though agreeing exactly in form, gave the observed time about half an hour later than the calculated time. The accordance between the theoretic curves and those derived from observation for the height, though they possessed a general similarity, was not nearly so great: this he conjectured might be accounted for in several ways.

He next proceeded to notice the inequalities due to the declinations of both luminaries: these, he stated to be so mixed up together that it is impossible to treat them in the same manner as those due to parallax. The London observations had been so discussed as to refer the time of high water to the next transit of the moon but three preceding the transit next before the time of high water. He had intended the same to be the principle of discussion for the Liverpool observations; but, after the work had been considerably advanced, he found that Mr. Jones had used the transit next but four previous to the one preceding the time of high water. By a simple method, which he had formerly explained, however, the argument was easily transposed from one transit to another; and this having been done by Mr. Russell, the London observations became at once comparable with those of Liverpool. A comparison of the London and Liverpool observations, in reference to the diurnal inequality for the same tide, gave a very curious result—it being found that the order of their inequality was completely reversed. The author did not think that this circumstance had been previously known; although he was aware that Mr. Whewell, in his examinations of the Coast-guard observations, noticed an anomaly, of which the origin is similar. The author also stated the fact as remarkable, that, while the diurnal inequality in the interval is almost inappreciable at Liverpool, at London it is well defined.

Mr. Lubbock here entered into an elaborate examination of the differential equations from which the form of the fluid ocean is to be obtained, and he pointed out the modification of the equations of the equilibrated surface of the fluid mass which should be adopted, in order to cause them to agree with the theory of Bernouilli, or, as Mr. Whewell has termed it, the Equilibrium theory; and he pointed out various desiderata, in order that the agreement of the two theoretic modes of investigation should be shown to be perfect. An improvement of theory, however, as far as regards single observations, or for the purposes of predication, he stated, was scarcely requisite, except as far as regards the fluctuations of the establishment, which arose partly from the inevitable difficulty attendant upon observations of the time and height of high water, and partly on account of the derangement produced by causes not yet subjected to the dominion of analysis: such as the winds, and variations of atmospheric pressure. In the averages, however, these almost entirely disappear; and, in general, the agreement of the phænomena with Bernoulli's theory is so exact and complete as to render it obvious, that its merits have not been sufficiently appreciated. He then exhibited diagrams, prepared by Mr. Russell, in which the curves, denoting the several circumstances of the tides, as deduced from theory, were distinguished from those given by the discussion of the observations, and, in almost every case, they accorded in a truly surprising degree, and were frequently perfectly identical. But it appeared, that the establishment and mean time of high water were still liable to slight fluctuations, which at present baffle all our attempts to obtain extreme accuracy in the prediction of tidal phænomena. The writer added, that, by a fortunate circumstance, the preservation of a register of the tides, kept for a short time during

the 13th century, at London Bridge, by an abbot of St. Alban's, John Wallingford, and which register had been preserved in the British Museum, it had been clearly shown, that the establishment for the port of London had varied since that time by a quantity extending to between two and three hours: the cause of this could be as yet merely conjectured.

Mr. Lubbock observed that he had thus called the attention of the Section briefly to those parts of the subject which appear to require further illustration, in the hope that some of the masters of the analytic art might turn their attention to the subject, and enable us to elucidate them; and he had given a summary of the more important facts which appear to result from these laborious calculations, which could never have been undertaken but for the interest which had been felt in the subject by some of the most distinguished members of the Association, particularly by Mr. Whewell, and for the grants of money which had been, at different times, devoted to this object from the funds of the Association; and he trusted that when the members saw the results published in full, as they would shortly be in the Philosophical Transactions, they would not deem their importance disproportionate to the great labour and expense which had been bestowed upon them. He also wished to inform the Section that he had lately received, through the kindness of M. Arago, the printed Brest Tide Observations from January 1807 to the end of December 1835, and the use that they should be put to remained to be determined. He had at one time been extremely anxious to obtain and to use them, partly from the difficulty experienced in using manuscript tables and the risk of injuring or losing them, partly because the Brest tide is single and readily compared with the similar London tide, partly because of the classical interest attached to them, they having been the foundation of the speculations on this subject by Laplace in his Mécanique Céleste, and because the Brest observations extend through the same period as those made at the London Docks, and which had been formerly discussed. But on the further consideration of the matter he was disposed to believe that little more could now be obtained from the Brest observations than had already been, from the comparison of the London and Liverpool observations; it would, however, be desirable to determine the semi-menstrual inequality in the height at Brest, which might be done from the observations of a single year, and it would be also desirable, no doubt, to compare the observations of identical tides at London and Brest which might now be done; but, on the whole, the labour and expense of a full discussion of them would be so great that he much doubted whether the advantages that would result would compensate for them.

Mr. Whewell congratulated the Section upon the aid which this difficult branch of science had now received, from the labours of the British Association; and the inestimable advantages which were likely to result from the employment of the funds of the Association on these subjects, an aid which they could not expect to obtain from any private source.

He then went on to observe that his own researches agreed with those of Mr. Lubbock, both in giving a very close and remarkable coincidence of the laws of observation and theory on most points, and also in disclosing some curious discrepancies of some of the features of the observed tides from the theoretical. In particular, he stated that he had satisfied himself, as Mr. Lubbock had done, but by independent investigations, founded on quite different facts, that the *diurnal inequality* was very different at different points of the same coast; and that at places not very distant from each other, he had found cases where this inequality was absolutely inverted, making *that* the lower of two successive tides, which, at a period of their progress a little anterior, had been the higher. He stated that, this circumstance having attracted his attention, he had, in a postscript to his *seventh* series of Tide Researches, printed in the Philosophical Transactions, offered a certain hypothesis as a mode of accounting for it—namely, that the tides might be conceived as transmitted by *transverse* undulations; and he added, that subsequent researches, about to be published in his *eighth* series, had shown him that he must entirely retract this hypothesis. He added also, that he was able to say the same of another hypothesis, at first sight very plausible—namely, that the diurnal tide travels at a *different rate* from the common semi-diurnal tide. He stated, that having taken sixty of the best-conditioned places on the coasts of Great Britain and Ireland for the purpose of tracing the progress of this diurnal inequality, he had had the requisite calculations made by calculators (Mr. Dessiou and Mr. Ross) placed at his disposal by the Admiralty. He had separated the *diurnal wave* from the semi-diurnal tide, by examining the comparative influence of the diurnal inequality upon high and upon low waters. He had pursued this diurnal wave *first* along the west coast of Ireland, round the north of Scotland, and down the east coast of Scotland and England; and he had found that the diurnal wave never gained or lost much in its rate of progress compared with the semi-diurnal. This was generally two or three hours behind—sometimes more than five, sometimes less than two, but with no *progressive* difference. He had *next* followed another diurnal wave up the Channel, and had found the same general approximation from the Land's End and Brest to the Isle of Wight; but in the Southampton waters, and so on to Portsmouth, the diurnal wave was thrown out of its course so much as to affect the tides in a reverse order to that which took place in the previous part of its course; so that if two successive tides, A B, progressed from Bridport to Southampton, A was higher than B at the first place, and B higher than A at the second. He referred also to the *double tides* (four in twenty-four hours) which occur in the Solent Sea, and invited the attention of persons residing in the neighbourhood of those coasts to the investigation of this subject, since such persons can best determine over what extent of coast this double tide prevails—how, at the extremities of its range, the double tide grows out of the single—at what intervals the two tides occur—which is the greater, and how these relations vary at different places,—and whether these changes can be connected in a definite manner with the tidal currents. He added, that in some places instead of four or two tides in the twenty-four hours, there appears to be only *one*, especially on the coasts of Australia. He observed, that he conceived he had already evidence to show that these supposed *single day tides* were, in fact, only extreme cases of great diurnal inequality; and he stated that the Admiralty, in pursuance of suggestions made by him, through Captain Beaufort, the hydrographer, had directed observations to be made at several points on the coasts of Australia, which he hoped would enable him to decide this question, and to draw from them the laws of such cases.

Sir W. Hamilton spoke of the valuable results of the laborious exertions of Mr. Lubbock, aided by the exertions of those who could not afford to give their time, or bestow their labour, without remuneration: but for the pecuniary aid, therefore, of the British Association, and the labours of her associate sons, those invaluable treasures, those hoarded masses of observations, would have lain for ever useless,—or, at least, until the paper on which they were inscribed had mouldered into decay. What advantage, for instance, could it ever have been to mankind, but for those laborious discussions, that it had been recorded, that, on the 14th of April, 1804, the tide stood at such a height at such an hour of the day or night?—what advantage was it, that these observations had become multiplied to an extent that was calculated to produce, in the mind of any person who looked over them, only a feeling of stupified confusion? But these very observations, when discussed, became like the rough ore from the mine refined and purified; and now they, indeed, become, if not the precious metal itself, at least the means of safely conveying to our ports the vessels which brought that wealth. By them, it became possible to predict, with almost mathematical certainty, the exact time at which the water in the harbour of this great commercial port would, on any stated day, attain a height which would suffice to float its proud navies over the shoals which lay around, and almost enclosed, the noble docks which had been prepared for their safe reception; and thus the merchants of this great port and of London became most deeply indebted to that very body on whose members they were now, in return, so lavishly showing their hospitable favours. Mr. Holden stated that he had, for many years, made the subject of the Tides on this coast his particular study. He could, at any time, by a very simple rule, deduce the time of high water, by dividing the period of the retrogradation of the moon's nodes by a certain divisor which we were unable to catch the value of.—Professor Stevelly stated, that the captain of the steam-vessel in which he had come from Belfast, and in which he begged leave here, in the way of parenthesis, to say, that her owners, Messrs. Langtry and Hardman had munificently permitted him and his brother associates to come free of expense—the captain of this vessel had mentioned some curious circumstances to him respecting the tides in some places along the coast of Ireland. The tide wave coming up the Channel was met about the Isle of Man by the tide wave which was propagated round the north coast of Ireland; and there a large tide was caused by the super-imposition of these waves; while, on the contrary, about the coast of Wexford, the half-tide of one wave was nearly met by the full tide of the other, so as there to produce the interference so much insisted upon by the opticians in their wave theory. On the coast of Wexford, accordingly, the tide scarcely rose two feet at full; while, on the other hand, round a neighbouring promontory it rose to the height of above nineteen feet. This promontory was so close to the town, that it had been at one time, for some purpose, in contemplation to cut through it; when it was ascertained, that if this were done, the houses were built so low, that they would all be laid under water every time the tide flowed. This high tide, perhaps, in that place, was caused by some particular reflexion of a partial wave of the Bristol Channel or of the Welsh coast.—Mr. Whewell stated, that he should be very happy if Mr. Stevelly could procure for him the exact particulars of these or other unusual circumstances connected with the tides; and it would be important to mention exactly the nature of the coast—as, for instance, whether the water was deep or shoal, and the land abrupt, shelving, or indented, &c.—Professor Stevelly undertook to obtain the particulars. He knew, indeed, that along the coast of Wexford an extensive sand-bank ran for several miles, causing a very dangerous shoal, which extended far into the Channel.—Mr. Lubbock, in reference to a part of the observations of Mr. Whewell, stated, that he found, that if, in the formulæ for the diurnal inequality, the argument for the port of London be taken from the transit of the moon six days previous, and for the port of Liverpool four days previous, the theory of Bernouilli will be found to give a very strict coincidence, when compared with observation.—Mr. Christie said, that in consequence of some unknown manner in which the tide waves were propagated round the Isle of Wight, the tides in the Southampton water were subjected to a very curious anomaly; for, about an hour and a half after the ebb tide began to make, a new, though diminished flood tide set in.—Mr. Snow Harris observed, that it was at Poole, he believed, that the effect of this second tide was most felt; and he had understood that the interval, instead of being only an hour and a half, was nearly three hours and a half.—Mr. Whewell said, that it was of the utmost importance that some intelligent amateur residing in the neighbourhood of the Isle of Wight should hunt these two tides completely round the coast, until the places of their appearance and disappearance, and of their greatest differences, both as to height and time, shall be clearly noted, and thus at length their cause ascertained. In the Indian Ocean, there were some very remarkable instances of such interferences of the tide wave; and Captain Fitzroy, lately returned from a voyage round the world, had investigated for him some of the anomalies of the tides, as well along the shores of America as upon the southern coast of Australia; and he had no doubt some important results would be elicited. On the south coast of Australia, in particular, one curious fact had already been ascertained—that there existed a very great difference between the diurnal inequality and that observed on the shores of Europe.—Mr. Harris explained the two tides at Poole on the principle, that the ebb tide making down the Southampton river, when it had already far advanced in the ebb tide at Poole, threw the current of the water over to that side of the Southampon Water, and thus caused a second rising of a flood tide in that neighbourhood. —Mr. Peacock congratulated Mr. Lubbock on the

very successful termination of his labours on this intricate, but most important, subject.

The President then called upon the Rev. W. Whewell, to explain to the Section the results of the observations made within the last year, by the aid of his Anemometer.

Mr. Whewell rapidly sketched the principle on which his instrument registered the quantity of aërial current passing any place. He had exhibited the instrument in an unfinished state at the Dublin meeting, and in a more matured state of its existence at Bristol; it had since received some valuable improvements, which were suggested by the practical working of the machine. That he might not occupy the time of the Section too long, it would suffice at present to say, that in it a small set of wind-mill vanes, something like the ventilators placed in our windows, were presented to the wind by a common vane, let the direction of the wind blow how it might: the aërial current as it passed set these vanes into rapid motion, and a train of wheels and pinions reduced the motion which was thence communicated to a pencil traversing vertically, and pressing against an upright cylinder, which formed the support of the instrument, and that 10,000 revolutions of the fly only caused the pencil to descend the one twentieth of an inch. The surface of the cylinder was japanned white, and the pencil as the vane wavered kept tracing a thick irregular line, like the shadings on the coast of a map: the middle of a line was readily ascertained, and it gave the mean direction of the wind actually exhibited before the eye by a diagram, while the length of the line was proportional to the velocity of the wind, and the length of time during which it blew in each direction; which therefore gave what he called the integral effects of the wind, or the total amount of the aërial current which had passed the place of observation in the direction of each point of the compass during the interval which had elapsed since the time of last recording the instrument. This, it was well known, was a subject of much importance in meteorological speculations, but has not been hitherto accomplished. It was indeed deemed of much consequence, to obtain even the mean direction of the wind at a given place, and the celebrated Kämtz, in his *Meteorologie*, has made a collection of several results of this kind; but, in the ordinary way of registering even the direction of the wind, which is, by stating the length of time it blows from a certain point of the compass; it is obvious that the velocity of the wind is altogether left out of account, and therefore the high wind or storm of one day, is placed on a par with the gentle breeze of the next, and therefore not an attempt can be made to infer the total quantity, or what he had ventured to term the integral effect of the wind. Mr. Whewell then proceeded to exhibit large diagrams, giving the results of the observations recorded at the Cambridge observatory, under the care of Professor Challis, and at the house of the Cambridge Philosophical Society. The similarity of the curves showed a general coincidence, but some discrepancies were accounted for by the circumstance, that the dome of the Equatorial sheltered the anemometers placed at the observatory on the north side, while that placed upon the house of the Philosophical Society was well situated for receiving the wind from every quarter. Anemometers on this principle had been also erected by Professor Forbes and Mr. Rankin, at Edinburgh, and by Mr. Snow Harris and Mr. Southwood at Plymouth; but he was not at present prepared to state the results of these observations, though he had little doubt they would be interesting and useful.

The President supposed it would rather suit the convenience of the Section to hear Mr. Osler give the description of his anemometer and rain-guage, before they proceeded to make observations on the communication of Mr. Whewell: accordingly,—

Mr. Osler, of Birmingham, read an account of a new Registering Anemometer and Rain Guage, now in use at the Philosophical Institution at Birmingham, illustrated by diagrams, giving a condensed view of the observations recorded during the first eight months of the year 1837.

He observed that although the results obtained by this instrument are essentially different from those produced by the Anemometer exhibited last year at Bristol, by Professor Whewell, he should have hesitated to introduce the one now submitted to the Section, had he not been kindly encouraged so to do by that gentleman himself. In this instrument the direction of the wind is obtained by means of the vane attached to the rod, or rather tube that carries it, and consequently causes the latter to move with itself. At the lower extremity of this tube is a small pinion working in a rack, which slides backwards and forwards as the wind moves the vane, and to this rack a pencil is attached, which marks the direction of the wind on a paper ruled with the cardinal points, and so adjusted as to progress at the rate of one inch per hour by means of a clock. The force is at the same time ascertained by a plate one foot square, placed at right angles to the vane, supported by two light bars running on friction rollers, and communicating with a spiral spring in such a way that the plate cannot be affected by the wind's pressure, without constantly acting on this spring, and communicating the quantum of its action by a light wire, passing down the centre of the tube to another pencil, below which it thus registers its degree of force. The rain is registered at the same time by its weight acting on a balance, which moves in proportion to the quantity falling, and has also a pencil attached to it recording the result. The receiver is so arranged as to discharge every quarter of an inch that falls, when the pencil again stands at zero.

Mr. Whewell spoke highly of the construction of this anemometer, and he had no doubt but that a very slight modification of the mode of registering its indications would cause it to answer every purpose which he had lately described as desirable. In its present form, however, it was the force of the aërial current, which it indicated, not the integral effect. He also highly commended the rain-gauge, and the method of showing in one diagram to the eye so many important meteorological phœnomena.—Professor Lloyd stated, that there was a very simple method of causing the anemometer of Mr. Osler to give the integral effect of the wind, and that was to cut out the paper covered by the tracings of the pencil indicating the force of the wind, and to weigh it; for it was easy to perceive, that since the ordinates of the curved spaces covered by those tracings were proportional to the force, and, therefore, the velocity of the wind, and the abscissæ to the time, the areas represented the integrals, or the total amount of the aërial current.—Mr. Ettrick asked, whether some other method of supporting the cylinder which moved back and forward as the force of the wind varied, rather than friction rollers, would not be desirable—such, for instance, as bridle rods, or other means known to practical mechanics, and, he was sure, well known to Mr. Osler. Mr. Osler replied, that many methods of supporting this part of the apparatus had been tried and laid aside, as not answering; among the rest, bridle rods.

Professor Powell then made a communication 'On the Dispersion of Light.' The object of this communication is to state the progress of the inquiry into the subject of dispersion since the last meeting of the Association. On that occasion, the author laid before the Physical Section the results of his observations for determining the refractive indices of the standard rays for 28 media. These have been since published, with some preliminary remarks, as one of the series of memoirs of the Oxford Ashmolean Society. They are to be considered only as first approximations, and it would be very desirable to have many of them carefully repeated, as well as to extend the inquiry to other bodies. The author regrets that he has been unable, from particular circumstances, to carry on these researches during the past summer, but intends to take the first opportunity of resuming them. In particular, he was kindly favoured by Mr. Brooke with a specimen of some chrystals of chromate of lead for examination, and accordingly put them into the hands of Mr. Dollond, who warmly entered into his views, and has used every endeavour to give them a prismatic form, but, unfortunately hitherto without success. It is only by such co-operation of those engaged in different departments of science that inquiries like the present can be successfully carried on, and the author is anxious to obtain specimens of any transparent media, which are capable of prismatic examination, and especially such as are of high dispersive power.

Meanwhile, he has been engaged in the comparison of observation and theory, especially among the more highly-dispersive bodies which he has examined. He has performed the calculations by the method of Sir W. R. Hamilton, and has found that for those media whose dispersion is not very great, the coincidences are sufficiently close; but, on proceeding to the more highly-dispersive bodies, especially oil of cassia, the discrepancies increase, and moreover preserve a certain regularity of character, which shows that they are not mere errors of observation: this would seem to warrant the expectation, that a further developement of the formula might still give successful results. These investigations have been communicated to the Royal Society, and will appear in the next volume of the Transactions.

Since the period of this communication, however, the able and profound memoir of Mr. Kelland appeared in the Cambridge Transactions. This gentleman's theory is, in some measure, a simplification of Cauchy's; the resulting formula, for the dispersion, though substantially the same, is developed in a different form, and readily capable of being applied to numerical computation. In some correspondence with Mr. Kelland, that gentleman favoured the author with a computation for the case of oil of cassia, in which the greatest discrepancies existed. By this method, those discrepancies have been made entirely to disappear; and thus the *most extreme case* at present known is brought under the dominion of the formula of dispersion. It is also to be observed, that Mr. Kelland's series is not rapidly converging; the neglected terms, therefore, *may*, if taken into account, give a still more accurate result. It will, therefore, now become of yet more extreme interest, to find some means of obtaining data for the more highly dispersive substances, such as chromate of lead, realgar, sulphur, &c. With regard to the theory, there may be much still wanting to render it entirely satisfactory. Its first principles have been discussed by several mathematicians, but especially in some papers read by Professor Lloyd to the Royal Irish Academy, embracing, in fact, the whole subject of the propagation of light in uncrystallized media.

Sir David Brewster observed, that some other method than observing the fixed lines of Fraunhofer, must be resorted to, for substances of such imperfect crystalline forms as those examined in this communication,—as, for instance, the chromate of lead. The method he would recommend, would be, either to interpose nitrous gas or plates of mica, so as to form a net-work; a given number of the colours of the resulting rings being then counted, and attended to in the various observations, would be much better than Fraunhofer's lines, which, indeed, in this case, he contended, would be altogether incapable of being accurately observed.

Section B.—CHEMISTRY AND MINERALOGY.

President—Dr. Faraday.
Vice Presidents—Professor Daniell, Professor Graham, Dr. Apjohn.
Secretaries—Professor Johnston, Dr. Reynolds, Professor Miller.
Committee—Mons. De le Rive, Dr. Thomas Thomson, M. Gay Lussac, M. Liebig, Dr. Yellowly, Professor Cumming, Mr. Richard Phillips, Mr. W. Herapath, Professor Edmund Davy, Dr. Andrews, Professor Clark, Mr. W. Lucas, Mr. H. H. Watson, Dr. Kane, Mr. Mackintosh, Dr. R. D. Thomson, Mr. Thomas Thomson, Jun., Mr. Charles Tenant, Mr. Crossall, Mr. Robert Mallet, Mr. Walter Crum, Dr. Ingles.

Dr. Apjohn, in the absence of the President and other Vice-Presidents, took the chair.—Prof. Johnston enumerated the different reports upon specific scientific questions confided at the last meeting of the Association to various members of the Chemical Section, and stated that some of them were prepared, and would be presented in the course of the week.

The first paper brought before the Section was one by Mr. G. Crane, of Yniscedwyn, 'On the Use of Anthracite Coal, by the combination of heated air to the purposes of smelting Iron Ore.' The reduction of the quantity of fuel expended to less than a third of that before required of the bituminous kinds for the production of one ton of pig iron—the increase of from forty to fifty per cent. upon the former make by this process, and the increased strength of the metal when compared with that before obtained by him from the native ores of the South Welsh Basin, with the use of the coke of the bituminous veins and cold blast, were the leading points of the paper. Mr. Crane dwelt on the abundance of this variety of fuel, of which there are large deposits in Wales, Scotland, Ireland, Sardinia, France, Transylvania, and particularly America.

The next paper was by Dr. Kane, of Dublin, and related to a series of compounds obtained from pyroacetic spirit. He stated the origin of pyroacetic spirit, and his reasons for considering it as a variety of alcohol, and expressed symbolically the composition of a series of compounds, quite analogous to those which, under similar circumstances, are yielded by alcohol. His communication, however, on this subject having been long since before the scientific public through the pages of the *Philosophical Magazine*, it is unnecessary to report it at greater length.

The Secretary then read a paper, by Mr. Golding Bird, 'On the Crystallization of Metals by Galvanic Influence.' To this department of knowledge popular attention has been peculiarly attracted by the well-known experiments of Mr. Crosse, detailed at the last meeting of the British Association at Bristol; and it is in connexion with this gentleman's experiments that the present paper is more particularly interesting. It has long been a matter of extreme interest and importance to connect those changes constantly going on in the physical world with those which are observed in the laboratory of the chemist—to compare the researches of the experimental philosopher with the effects everywhere produced in the vast amphitheatre of nature. With this view the experiments about to be detailed were undertaken. Philosophers have long been accustomed to attribute the formation and crystallization of metals in mineral veins to voltaic action, but this could be regarded as little else than a matter of assumption until some experiments actually supported this point of view. To M. Becquerel we are mainly indebted for the knowledge of the power of a single galvanic circle in producing powerful voltaic decompositions, whilst to our own countryman, Dr. Faraday, we owe that most important piece of information, that poles, or *attracting surfaces*, are by no means requisite to the crystallization of a metal, and that all that is necessary for the reduction of a metal from a salt or oxide is the mere passage of a voltaic current. That this current may be of the weakest intensity has been shown by Dr. Bird in an essay lately read before the Royal Society of London. The apparatus contrived by Mr. Bird was very simple, consisting of an external cylinder of glass, capable of holding about half a pint of fluid, filled with a solution of common salt (chloride of sodium); into the contents of this cylinder was plunged a second and smaller cylinder, furnished at its lower extremity with a plug of sulphate of lime: this second glass cylinder was filled with a solution of sulphate of copper; into the latter a plate of copper, furnished with a conducting wire, was immersed, whilst into the solution of salt a plate of zinc, also furnished with its conducting wire, was plunged. Under these circumstances, a current of electricity is developed, the plate of zinc becoming positive, and the plate of copper negative, although the *intensity* of the current could be scarcely supposed sufficient to the production of chemical action. Mr. Bird has, however, shown, that when the connecting wires of the two plates of this elementary battery were immersed in a saline solution of a compound salt, the most important physical and chemical changes were produced; and that if, instead of immersing these wires in fluids, they are twisted together, so as to insure metallic connexion, it will be found that the electric current developed will produce most interesting and unexpected effects on the metallic solution present in the smaller; for, although it might be anticipated that the copper would be reduced, yet we should expect that this reduction would be most obvious at the surface of the negative electrode, which, however, Mr. Bird has shown not to be the case; for, on examining the plug of sulphate of lime (plaster of Paris), closing the smaller cylinder, and separating the solution of sulphate of copper from the brine, it was found that beautiful and hard crystals of metallic copper were deposited in it, not in a confused manner, but in veins precisely resembling those met with in mines, of which, however, it is scarcely necessary to observe they presented but a miniature resemblance. From this, it appeared, that the *mere passage of an electric current*, independent of the presence of poles, was sufficient to effect metallic reductions, supporting, in a satisfactory manner, the experiments of Dr. Faraday on this subject. The metallic crystals thus obtained were very hard and brilliant, resembling in a striking manner those produced in the vast theatre of nature—indeed, some specimens exhibited by Mr. Bird, obtained by the aid of his miniature apparatus, precisely, and, indeed, so closely resembled the most perfect forms of native and ruby copper ore, that they would probably defy the most expert mineralogist to discover their true origin. These effects were, moreover, by no means confined to salts of copper; for, when solutions of antimony, lead, tin, zinc, bismuth, silver, or other metals, were placed in the inner vessels, instead of a solution of copper, the metals were, in every case, reduced, partly on the plate of copper which served for the negative electrode, but chiefly in crystals imbedded in the mass of plaster of Paris closing the inner cylinder.

Other experiments, bearing upon this subject, were also detailed, which it is unnecessary to mention. They, with those already noticed, were considered interesting in explaining the cause of the deposition of metals in veins; for, as the magnetic theory of Arago, Ampere, and others, requires that free currents of electric matter should be perpetually circulating our earth in a direction at right angles to the magnetic meridian,—so these currents, instead of merely causing the evolution of magnetic phenomena, are shown to be sufficient to produce most important chemical changes, causing, by their passage through masses of clay or earthy matter, the reduction and crystallization of the metals diffused through them in solution. To one circumstance, Mr. Bird particularly called the attention of the meeting—viz. the danger of considering the chemical changes produced in the bowels of the earth as in the first place depending upon metallic veins themselves; for, although it was evident that by the action of heat upon them, thermo-electric currents may be, and no doubt are, developed, yet we must regard the first physical cause which induced the deposition and formation of these very veins; and this cause, it is evident, can be none other than, in the first instance, chemical action. Upon this point, Mr. Bird's experiments, in conjunction with those of Dr. Faraday and M. De la Rive, are certainly interesting, as throwing light upon that most obscure of subjects—the formation of metallic veins in the bowels of the earth.

A valuable part of this communication appeared to us to be, that in which Mr. Bird suggested that the silicification of wood is an electrical phænomenon. He has undertaken experiments to test this theory, and we are happy that so interesting a subject of inquiry should be in such competent hands.

SECTION C.—GEOLOGY AND GEOGRAPHY.

President—Rev. Professor SEDGWICK,
(*For Geography*) Mr G. B. GREENOUGH.
Vice Presidents—Mr. LEONARD HORNER, Lord COLE, M.P., H. T. DE LA BECHE, F.R.S.
Secretaries—Captain PORTLOCK, R. HUTTON, Esq., M.P.,
(*For Geography*) Captain H. M. DENHAM, R.N.
Committee—Mr. James Bryce, Sir Philip Grey Egerton, Mr. W. J. Henwood, Mr James M'Adam, Major Sabine, Mr. H. E. Strickland, W. West, M.D., Mr. H. T. M. Witham, Rev. M. Dwyer, of Liverpool, Mr. W. B. Clarke, Mr. Richard Griffith, Mr. B. Ibbotson, Col. Silvertop, Mr. R. I. Murchison, T. S. Traill, M.D., Rev. James Yates, Professor Phillips, Major Shadwell, Chester.

This Section, as we announced, held its meetings in the theatre of the Mechanic's Institute, in which there is a gallery; a number of ladies were consequently enabled to be present at the meeting. It was opened a little after 11 o'clock by the President, Mr. Sedgwick, who excused himself and the committee for a want of punctuality, owing to some of the arrangements not having been completed.

The first communication was from Mr. Whewell, 'On the Changes of Level of Land and Water,' which have taken place, or may be likely to do so. He mentioned that this was one of the questions proposed by the Association, and for which a grant of money had been voted,—namely, to ascertain with great accuracy the differences of level of a number of points in two straight lines, at right angles to each other, and terminating on the sea coast. He referred to some of the many indications of change of level, which may be observed in different parts of the world, especially the occurrence of elevated beaches; and alluded to the ease of determining changes of level in seas having little tide, as in the Baltic, compared with some of the parts of the coasts of Great Britain, where the tide rises very considerably. He then mentioned, that although a portion only of the task prescribed had been performed, he could vouch for the accuracy of the survey which had been made in a line from Bridgewater to Axmouth. The gentleman who effected this survey was Mr. Bunt of Bristol, and the line is the same as that chosen formerly for a ship canal, between Bristol and the south coast of Devon.

Mr. Smith, of Jordan Hill, called the attention of the Section, to a paper he had lately communicated to the Geological Society, respecting changes of level of land and sea in the west of Scotland, and stated that he should be glad to communicate, if required, the information he possessed on this subject, at any of the sittings of the Section. Mr. Sedgwick remarked on the future utility of observations made with accuracy at the present time. The line determined by Mr. Bunt would serve as a kind of base to work from, and the continuation of the survey would bring to light many facts, that might be used with advantage by subsequent observers.

The next communication brought forward, originated also from a question proposed by the Association at a former meeting, and submitted to a committee. This committee undertook to make experiments, for the purpose of determining the quantity of silt in sea water at different depths; but of their number the only gentleman who had performed an extensive series of experiments, was Captain Denham, of Liverpool, who now made a report to the Section, of which the following is a brief outline.

He stated that the proportion of insoluble matter contained in the Mersey, amounts to twenty-nine cubic inches in the flood, and thirty-three inches in the ebb, in each cubic yard of water; evincing a preponderance of one in eight in the matter of the ebb, or 48.065 cubic yards of silt, &c. which is detained by the banks outside the Rock Narrows each tide, with the exception of what the succeeding ebb disturbs, at the exhausted stage of the former ebb. Thus, the ebb of to-day ranges over sixty-four square miles, and the next ebb over forty-four square miles, reducing by one-third the first day's layer, that being the relative proportion of silt held in solution, and deposited over the outer area, at the northern margin of which the cross-set of the Irish Channel ebbs, limits the deposit by sweeping into broad water what may extend so far. Now the excess of silt, on the 730 refluxes of tide that occur in a year, amounts to 35,087,450 cubic yards, capable of spreading a layer, if equally disseminated, of twenty-one inches thick over the first tide area; one-third however is disturbed, and carried over the second tide area; or there is an uniform increase of the banks, and decrease of water in the channels of the estuary of the Mersey, amounting to seven inches per annum. This deposition of matter is however very unequal, some parts of the coast and of banks receiving great accumulation, while others are often taken away. At the quarantine ground, the bed of the river shoaled up twenty-two feet in eight years, and then eleven feet in two years, over a space of half a mile long by one quarter of a mile wide, and yet this was swept away in eighteen months. Captain Denham had been examining the port of Liverpool for fourteen years, and he infers from his observations, that a time will arrive when no access to this port could exist, unless man sets bounds, by his ingenuity, to the operation of tidal action. He made a number of local observations, which showed the diligence he had exercised in both planning and executing whatever he conceived might benefit this most important port; and he finished by an explanation of his principle of a constant sea level, which he had ascertained to be at three hours before, or three hours after high water, and by exhibiting the instrument which he had employed in drawing up water from different depths.

Mr. De La Beche pointed out the great importance of these experiments, both to the scientific and the practical man, in affording information concerning operations and works on the banks of estuaries and rivers. He conceived that the utmost caution should be used in making embankments, as in most instances they had proved detrimental to the navigation.—Mr. Yates spoke also of the enormous quantity of silt determined by Captain Denham, as being of vast importance in illustrating the opinions of Mr. Lyell, on the changes produced on the earth's surface, by causes still in operation. He alluded to a communication made some time ago by Mr. Horner on a similar point, namely the quality of earthy matter contained in the waters of the Rhine.—Lord Northampton

gave Captain Denham's experiments as an example of the importance of the British Association, and of its peripatetic character. The important port of Liverpool has been ever in great danger of having its navigation seriously injured by the accumulation of silt at the mouth of the Mersey. In consequence of a question proposed by the Association, the true nature of the case had been discovered, and remedies pointed out.

The President called on the Rev. Mr. Yates, who exhibited to the Section some interesting remains of fossil vegetables found in the new red sandstone of Worcestershire. He mentioned the discovery of similar remains in the same formation in other parts of England—as at Coventry, where trunks of trees, of a considerable size, had been found; and stated that in the Royal Institution of Liverpool is preserved a fossil trunk, found in excavating Prince's Dock. The specimens laid before the Section were from two quarries between Worcester and Ludlow—one in the parish of Stanford, and the other in Ombersley; the former being where the new red sandstone joins the Silurian rocks. In this quarry the stone is rather greenish, like coal sandstone, and not unlike the Keuper of the Germans; but it may be traced, in a line of about ten miles, into sandstone of the usual red colour. In the second quarry, branches of trees have been discovered, and trunks partly converted into coal: each trunk seems embedded in a cylindrical mass of ferruginous matter. Through this quarry, a trap dyke passes, altering the rocks on each side. A specimen of the metamorphic rock, encrusted by chabasie, was exhibited. The general appearance of the quarry led Mr. Yates to the conclusion that there had been a deposit formed by a current near the shore of a sea, which deposit had been fixed in a bay or recess, where the remains of vegetables lay without being disturbed; and he alluded to the banks at Liverpool, where scarce any drifted plants had been discovered, owing to the continued motion of the currents, while they might be found in coves along the shores where the water was less agitated.

Mr. Murchison was then requested to answer a question respecting the identification of this sandstone with some of the divisions proposed by continental geologists. Mr. Murchison stated, that, in connexion with Mr. Strickland, he had pointed out the identity of the variegated marl, or upper stratum of the new red sandstone, in the counties of Warwick, Worcester, and Gloucester. In this stratum they had observed a thick band of sandstone inclosing a peculiar bivalve shell, not yet determined—the sandstone also contains remains of fish, and perhaps of Saurians. They were able to determine the superposition of these beds over the sandstone described by Mr. Yates, and also a total difference in the embedded fossils. Mr. Murchison considered that he was warranted in referring Mr. Yates's sandstone to the *Grès bigarré* of the French, and the upper beds to the Keuper of the Germans. In this opinion he was the more confident, as M. Adolphe Brongniart had identified a perfectly distinct series of vegetable remains in these two divisions of the sandstone formation. The opinion of Dr. Buckland respecting the age of some of the Warwickshire beds, must be considered premature, as his data were very insufficient: indeed, the Saurian remains, which had been so important an argument in favour of the supposition, were no longer to be found. Mr. Murchison referred to Dr. Lindley, who described to the Section the characters of the fossil plants discovered in the English formation of *Grès bigarré*. A discussion then took place respecting the ages of the new red sandstone divisions. The Rev. Mr. Clarke exhibited specimens of vegetable remains inclosed in new red sandstone from America, transmitted to him by Professor Hitchcock, who discovered the marks of the steps of birds in this rock a short time since. In a letter to Mr. Clarke the Professor has notified further discoveries of these singular steps, and also the remains of Saurians in this formation. In greywacke he had also discovered the remains of Marsupial quadrupeds.—Mr. Sedgwick conceived the classification of the different parts of the new red sandstone series as one of the most important points to be determined in the science. He pointed out a lower member of the series that immediately overlay the coal, although but rarely conformable to it: he has found in it coal fossils, as Stigmaria and Lepidodendron; he had observed it lately at Kendal; he considered it highly important to determine this member of the series, as well as the *Grès bigarré*, which lay above it. There was a difficulty in doing so in this country, from the absence of the Maschelkalk; but the fixing the proper place of the members of this new red sandstone series in general was of the utmost importance, as being the base on which all geological classification was founded. The prevalence of much of the red oxyde of iron in rocks was also unfavourable to the preservation of organic remains, so that we often want this evidence in this series. Mr. Sedgwick mentioned that Liverpool was seated on the *Grès bigarré*; and, although clay galls were often found in the rocks, it did not invalidate his opinion, as it has its beds of marl as well as the Keuper; even in old red sandstone in Scotland he had detected the galls. In answer to an observation of Mr. Greenough's, that there was a doubt respecting the coal sandstones, Mr. Sedgwick mentioned there were often strata of red sandstone mixed with the coal itself, and that there were many local peculiarities which might mislead an observer of England only; but, in comparing English Geology with that of Europe, we must sink local distinctions, and endeavour to generalize our views. The same lower red sandstone which is compact in Cumberland, becomes loose sand in Durham—and this anomaly would mislead a local observer, were the comparison not extended to the continent. It was stated by Dr. Lloyd, that he had found three teeth in the sandstone at Warwick—that he had examined attentively the formation supposed to be Keuper by Dr. Buckland, that he had observed alternations of marl and compact sandstone at different places, especially at Hartsill, where he had observed a limestone, also, that might be Maschelkalk; but he was unwilling to pronounce a decided opinion.—Mr. Murchison did not consider this limestone as Maschelkalk, although he had observed a limestone at Broughton, in Shropshire, which corresponded in position, but, unfortunately, was deficient in organic remains. He conceived it still possible to determine accurately the distinction between Keuper and *Grès bigarré* without the presence of this rock, as in the south of France, where Dufresnoy had completely established the distinction. What Mr. Lloyd had observed, he conceived to belong to the portion called by the Germans *rothe todte liegende*.—Mr. Stutchbury mentioned that he had examined the teeth discovered by Mr. Lloyd; and that, so far from referring them to the Keuper, he conceived they belonged to a stratum under even the *Grès bigarré*.

SECTION D.—ZOOLOGY AND BOTANY.

President—W. SHARPE MACLEAY, F.L.S.
Vice Presidents—Dr. RICHARDSON, Professor GRAHAM, Professor LINDLEY.
Secretaries—C. C. BABINGTON, F.L.S., W. SWAINSON, F.L.S., Rev. L. JENYNS, F.L.S.
Committee—Rev. Thomas D. Hincks, L.L.D., N. A. Vigors, F.L.S., Rev. F. W. Hope, F.L.S., Pat. Neill, L.L.D., Professor J. S. Henslow, T. S. Traill, M.D., Earl of Derby, Rev. W. Hincks, F.L.S., John Curtis, F.L.S., P. B. Duncan, F.L.S., J. E. Gray, F.R.S., Mr. C. S. Parker, Rev. J. Yates, Messrs. J. E. Bowman, T. Eyton, J. P. Selby, C. Horsfall, R. Ball, L. W. Dillwyn, J. N. Walker, A. H. Haliday, J. T. Mackay, Capt. Jas. Ross, Sir W. Jardine, Messrs. R. Harrison, Tinne, H. Sandbach, J. Salisbury, —Green, Dr. Duncan, Messrs. F. Archer, G. Cook.

W. S. Macleay in the chair.—Dr. Traill exhibited a specimen of the *Argas Persicus*, or Poisonous Bug of Mianneh in Persia, giving a short notice of its effects. In some parts of Persia it is the prevalent belief that this animal not only produces fever, but often death from its bite. It is not a true insect, but belongs to the order Arachnidæ, and to the genus Argas, from which it was separated by Lamarck. Two districts in Persia are largely infested with it, and it is reported that to sleep exposed in these is certain death.

Dr. Bell, a resident, had never known a case in which death was produced, but had seen persons extremely ill from its effects.—The Chairman doubted whether there was sufficient authority to believe that the bite of the insect was mortal, and ascribed the dangerous effects to the inflammation produced by pulling out a serrated proboscis, and stated his opinion, that death would not be produced unless in a diseased and excitable habit of body.—Dr. Traill stated, that its fatal effects had been positively mentioned by Sir R. K. Porter, Mr. Morier and other travellers. During the time that General White was Envoy to the Persian Court, the Schah dispatched a messenger after him, who requested him not to pitch his tent on a certain part near the city, on account of the bites of the insects.—Rev. Mr. Hope referred to a similar species in St. Domingo, which attacks horses in the ear, and often proves destructive; and the Chairman observed that it was rare to see a drove of oxen in Cuba exempt from the attacks of noxious insects, but which, instead of being prejudicial, were considered beneficial to the animals.

Mr. Gray drew attention to a new Water Lily of a gigantic size, sent over by Dr. Schomburgck from Guiana, and named *Victoria Regina*.—(See *Athenæum* of Saturday last).

Mr. Gray offered some remarks on the supposed production of insects, by the experiments of Mr. Crosse, and referred to two experiments made by Mr. Children in a manner perfectly identical with those of the former. The solution of silica was obtained from Mr. Garden, in Oxford Street, and in one experiment it was sealed up, whilst in the other it was exposed to the air, but in neither case was there any appearance of insects. The insects had been very indefinitely described by Mr. Crosse, some having six, and others eight legs. It was no proof that they could not have been produced from the water used in the experiment because it was boiled, as that would not be sufficient to destroy the eggs of the insects deposited therein.—Rev. Mr. Hope remarked one peculiarity, that no one had given the insects a specific name, and that they merely appeared to belong to the commonest species of Acari.—The Chairman mentioned the circumstance, that the seeds and germs of animals and vegetables are earlier and more quickly developed in a current of electricity, and that in all probability, these favourable circumstances operated upon the eggs of the insects produced in question. It was well known that seeds would retain their vitality for an indefinite period of time, and there was no reason why any limit should be put to the vitality of the eggs of animals.—Mr. Gray stated that prussic acid had lately been used for the purpose of destroying insects at the British Museum, particularly those infesting a mummy. Some of the larvæ of the common Musca having been put into the acid, remained uninjured after two or three days' exposure.—Professor Graham remarked, that other plants and animals might be kept for an indefinite length of time, when the powers of life were either retained or suspended. He also alluded to some curious experiments recently made at Edinburgh, although first by Sir Astley Cooper in London, with respect to the circulation of blood through the brains of particular animals. If the circulation be suspended by pressure for half a minute, the animal becomes torpid, but after giving a few convulsive sobs recovers, whilst if it is suspended for a minute the animal irrecoverably dies.—The Chairman observed that he had often dried to powder the eggs of various insects, which having been put into water were hatched.

The Rev. Mr. Hope read a letter from Sir Thomas Phillips, 'On a Method of destroying Insects which affect Books and Manuscripts, particularly the *Anobia*.' For the purpose of preserving books, he had used paste, in which corrosive sublimate was mixed, which would for some time resist their attacks. He had effected the destruction of *Anobium striatum* in his library, by placing in different parts of it pieces of beech plank, smeared over in the summer with pure fresh paste. It was soon discovered which pieces of the wood were infected, by the sawdust, and these were removed and burnt. So injurious is this species, that he considered that one impregnated female would be sufficient to destroy a whole library. He had also observed two other enemies—a small brown beetle; and one much larger, introduced from Darmstadt or Frankfort-on-the-Maine, which was not very abundant, although very destructive. This latter was about six times the size of the former, of a black colour, with white spots or stripes, belonging to the modern family Curculionidæ, and being most partial to books bound in oak boards.

Mr. Curtis suggested the employment of spirits of turpentine, as the effect of corrosive sublimate, and other poisonous substances, only lasted a short time, and soon stained the leather.—The Chairman remarked on the destructive effects produced by

Dermestes in his library in Cuba. It was probable that the insects which attacked the paper were different from those which attacked the paste, the former being Acari, and the latter small coleopterous insects. He had found no method of preservation so effectual as to give the books a free current of air, and, for this purpose, he was always accustomed to leave his book-cases open, the books being placed about two inches from the wall, so as to allow a free circulation.—Mr. Hope remarked, that the infusion of quassia had been esteemed a preventive; and Mr. Gray stated, that, in Geneva, the water used in the manufacture of paper was that in which quassia had been infused.—Mr. Golding Bird referred to the observations of Mr. Gray, with respect to the production of insects, as stated by Mr. Crosse in his experiments, which he had repeated on a large scale, but without any result, although he had continued them for some weeks, varying them in every possible form. He also explained that such could not have been produced from the silica, as this was precipitated from the mixture of the alkaline solution of silica and muriatic acid, the fluid passing through the filter being nothing but very dilute muriatic acid.

Mr. John Ball made some observations on the *Erica Mackaina*, described by Mr. Babington about two years since, and presented some specimens. It is confined to a district of hornblende rock. One characteristic peculiarity, which has not before been noticed, is, that in the *Erica Tetralix* (to which the present species is most nearly allied), at the upper part of the flowering branches, the whorls of leaves become gradually more remote, the upper part of the branch being bare; while, in the present specimen, the leaves continue in crowded whorls to the summit of the branch. The two plants are found growing together, preserving their distinctive peculiarities of habit.—Professor Graham was inclined to think that the present specimen was nothing more than a variety of the *Tetralix*. He was very much opposed to the present system of extending our specific nomenclature.

After some further observations, by the Secretary and other gentlemen, on the difficulty of determining between species and varieties, Professor Henslow referred to a suggestion, made at a previous meeting of the Association, that certain seeds should be selected, where opportunity occurred, which should be brought to the meeting, and distributed to different parts of the country, so that it might be observed what effect was produced by different circumstances of soil and management, and the constant characters assumed by plants might thus render their nomenclature less difficult.

Section E.—ANATOMY AND MEDICINE.

President—William Clark, M.D.
Vice Presidents—James Carson, M.D. F.R.S., Peter Mark Roget, M.D. Sec. R.S., Robert Bickersteth, Esq., Professor R. T. Evanson, M.D. M.R.I.A.
Secretaries—James Carson, jun. M.D., J. R. W. Voss, M.D.
Committee—Neil Arnott, M.D. F.R.S., Richard Bright, M.D. F.R.S., Hugh Carlisle, M.D., James Copland, M.D. F.R.S., Professor Richard T. Evanson, M.D. M.R.I.A., Richard Formby, M.D., Augustus B. Granville, M.D. F.R.S., John Houston, M.D. M.R.I.A., James Johnson, M.D., Jas. Macartney, M.D. F.R.S., Charles Herbert Orpen, M.D., William Henry Porter, Esq., Charles B. Williams, M.D. F.R.S., John Yellowly, M.D. F.R.S.

Dr. Roget stated that he presided on this occasion, owing to the absence of Professor Clark, whose arrival was expected.

The second report of the Sub-Committee, appointed by the Association to investigate the Motions and Sounds of the Heart, was read by Dr. Charles Williams.

Before describing their last investigations, the Committee stated that they had found frequent opportunities of confirming the conclusions of their former researches on the natural sounds of the heart; and these conclusions not having been shaken by any subsequent experiment, or well-founded objection, the Committee consider them established; *viz.*, that the first sound of the heart is *essentially* caused by the sudden and forcible tightening of the muscular fibres of the ventricles when they contract; and that the second sound essentially depends on the reaction of the arterial columns of blood on the semilunar valves of the arterial orifices, at the moment of the ventricular diastole. Certain other circumstances were stated, as being capable of adding to, or modifying these sounds.

The chief subjects of their present inquiry were, the unnatural, or morbid sounds, sometimes heard in the heart and arteries; and in investigating the causes of these sounds, which Laennec compared to blowing, filing, sawing, purring and cooing, or musical sounds, they sought to determine, 1st, What is the essential physical cause of these sounds; and 2nd, In what manner disease can develope this physical cause—a correct answer to these inquiries would determine the value of these sounds as signs of disease.

The Committee found that they could produce precisely the same sounds in every variety, by impelling, in various modes and degrees, a current of water through India rubber tubes; and by numerous experiments, they ascertained the relations which the character of these sounds bore to the nature of the impediment, and to the force of the current. They obtained similar results on experimenting on the arteries of living animals; and discovered, that in the human subject the same sounds may be produced by simple pressure, not only in the arteries, but in the veins also. They found that the sounds heard in the neck, described by some eminent French writers under the names, "bruit de diable" and "bruit de mouche," as signs of a particular morbid condition, which requires the use of certain remedies, may be produced at will, by the pressure of the stethoscope on the jugular veins of the healthiest persons, and is therefore not necessarily a sign of disease, but has probably been accidentally caused by the same pressure in many cases in which it has been considered as a morbid sign.

The Committee conclude, in answer to the first inquiry, that a certain resistance to a moving current is the essential physical cause of all the various sounds in question, and that this resistance is generally given by some pressure on, or impediment in, the tube through which the current moves; but that sometimes the resistance is caused by a change in the direction of the current, by which it is made to impinge on the walls of the vessel which contains it.

The second inquiry the Committee think can be fully answered only by extensive clinical and pathological observation, with due regard to the previous investigations; but they have planned some experiments, that promise to elucidate certain obscure points of the pathology and diagnosis of diseases of the heart and arteries, the knowledge of which would be of direct practical advantage. These points the Committee propose to investigate, if the Association think fit to re-appoint them to this office.

The thanks of the Section were voted to the Sub-Committee, and the members were requested to continue their labours.

Dr. Copland said, the Report stated that the heart beat after the cessation of respiration, which he believed, for the heart was the *ultima moriens*.—Dr. Carlisle concurred with the Report in its general view as to the cause of the sounds of the circulation in tubes from obstruction to the course of the fluid current, but differed from them on the explanation of the systolic sound, which, in his opinion, was owing to resistance from the irregularities of the heart's surface. The heart, in these cases, was *ultima moriens*, and would act, though it contained no blood, contrary to Bichat's opinion.—Dr. Granville could confirm, from his experiments with poison, chiefly prussic acid, the solitary instance adduced by Dr. Williams, of the beating of the heart on the cessation of respiration, and that it continued its action, though without blood. He found, and five other gentlemen were present timing with their watches, that in seven experiments on animals made in London, the fibres of the heart were alive after the death of the lungs (i. e. if life could be pronounced from dilatation and contraction)—in one case five minutes, in a second eight minutes, in a third eleven minutes; and on touching with a scalpel to a much longer period; and in this case not a drop of blood was contained in the heart. The action was not owing to blood or fluid, but was merely mechanical; and it was found, in dogs and cats destroyed by prussic acid, that, on opening the cavity of the chest and abdomen simultaneously, respiration had ceased, the lungs collapsed, but the heart beat, and the intestines acted peristaltically. The heart, as a muscle, does most certainly survive respiration, and is the *ultima moriens*.

Dr. Johnson said the Report concluded that all abnormal sounds were indicative of disease or morbid obstruction; from which he must dissent. He believed the excess of the velocity of the circulating fluid was sufficient to account for it, without the supposition of any obstruction from morbid contractions, and other impediments in the arterial tubes. He was, himself, an instance of the truth of this; for when his circulation was increased, either by excitement, food, or running, he had heard these sounds in himself, and he had equally verified it by observations on others; he could not, therefore, consider these sounds as certainly abnormal. He would ask Dr. Williams, if a trial had been made of the effect of velocity of fluids through tubes with the view of ascertaining sounds?—he would ask, if sound was produced at the extremity of the vessel, why is it not heard in the diastole of it, the blood issuing, as some say, in jets, or from reflux?—Dr. Williams stated, in answer, first, that the Committee had examined this subject physically, when they found some contraction, or obstruction, or bend, was necessary for the production of the murmur and other sounds; but that they had not entirely sifted the physiological part of the subject, though he believed that their physical labours were applicable fully to the physiological. To have sounds in currents, there must be some resistance, as friction. In the natural condition of the vessel, this increased velocity of Dr. Johnson's might create friction, from want of dilatability causing the murmuring sounds; and he would certainly recommend Dr. Johnson and his friends not to repeat their experiments frequently—at least, on themselves. To Dr. Johnson's second question—why is there not sound in a continuous current?—it would depend on the pressure; according to its degree, so would be the sound—if considerable, the *bruit de diable*. He now wished to refer to the objections of Dr. Carlisle. In last year's Report, they attributed the essential sounds of the heart to the muscular action of the heart, not to the blood; for they found the sounds perceptible, but in a diminished degree, without blood. In answer to the question, why the sounds of the heart should differ from those of the artery, he should say, the motion of the heart was only commencing, had not yet acquired momentum, and the blood was expanded over a large surface—that the ventricle contracted towards a centre, which was the artery—and that there was very little friction on the internal surface of the heart, though he could not tell how much to attribute to the *Carneæ columnæ* alluded to by Dr. Carlisle; but if examined, they would see very little irregularity where the blood converged, the principle being towards the bottom. The blood run with a different momentum in the arteries than it did in the heart, being only about three inches in half a second, and the slightest obstruction must, therefore, oppose it, while in the heart the blood moved as the muscle of the heart contracted, and other obstructions moved. As to the character of the sounds, they resembled those of natural objects, and the natural differed from the morbid. The natural was a dumb percussive sort of sound, the morbid was whizzing, giving an idea of friction—filing, sawing, rushing, and so on, which an accustomed ear could distinguish with precision.—Dr. Williams replied to a question asked by Dr. Granville, that the general result of their investigations led to the conclusion thta velocities of motion did not alone produce sound; there must be some obstruction, some resistance.

Mr. Brett then read a paper 'On the Physical and Chemical characters of Expectoration in different Diseases of the Lungs, with some Preliminary Remarks on the Albuminous Principles existing in the Blood.'

The remarks on the blood referred more particularly to a general view of the albuminous principles existing in that fluid. The simplest view which could be taken of the vital fluid, is that which refers its constitution to a mixture of fluid, or soluble, and insoluble albumen,—the one constituting what is termed the serum, the other the crassamentum, or cruor. The author of the paper then proceeded to relate the different opinions which had been published on the specific gravity of the blood, quoting the statements of Berzelius, Gmelin, Dumas, and other chemical philosophers of distinction; at the same time remarking, that all these statements did not differ materially from each other, and might be considered as depending upon the fact, that the specific gravity of the blood might differ slightly, not

only in different individuals, but in the same individual at different times. He then noticed the different modifications of albumen existing in the serum, which he divided into three forms:—1st. Soluble or free albumen, capable of undergoing coagulation by heat; 2ndly, Albumen in combination with a basic body, viz. soda; and, 3rdly, A form of albuminous matter, which he termed the colourless self-coagulating albuminous principle.

The crassamentum, as it is commonly called, of the blood, he also considered as made up of more than one form of *solid albuminous* matter; viz. of solid albumen capable of undergoing decoloration by ablution with water, and of solid albumen incapable of being decolorated by the same process; the former being insoluble, and constituting what is commonly understood under the name of fibrine, the latter soluble in water, and frequently designated red particles or *hæmatosine*. Some remarks then followed on the microscopic examination of the blood, and on the different forms of the globules in different animals. The author then proceeded to detail the various physical characters of the expectoration in the healthy condition of the lungs, as well as in its varied morbid states. The physical characters of saliva were entered upon, and the globularity of its opaque portions alluded to. The physical characters of expectoration in the pituitous catarrh of Laennec were then detailed; also those of the expectoration in acute and chronic bronchitis—in hæmoptysis, or pulmonary apoplexy—in pneumonia—and lastly, in different stages of phthisis. The chemical characters of these different modifications of expectoration were then fully treated of, and reference made to a tabular arrangement which the author had embodied in his papers, exhibiting the action of certain re-agents—first, on saliva, and then on the different forms of sputa, the physical characters of which had been already fully noticed. It was remarked, that saliva did not contain any soluble albumen capable of undergoing coagulation by heat; neither did it contain any solid albuminous matter, the main bulk of the solid contents of that secretion being mucous. The mode of analysis adopted, was, to deject saliva in cold water, and then subject the filtered fluid to the action of certain re-agents; another portion of saliva was then dissolved in a caustic alkali, and the alkaline solution subjected to the agency of certain tests. The quantity of solid matter in a given weight of saliva was also announced, as well as the saline matters, and their chemical nature stated: different authorities bearing on the subject were quoted, especially the statements of Berzelius and L. Gmelin. The chemical characters of expectoration in pituitous catarrh were then described, and a mode of analysis was stated to have been adopted, analogous to that employed in the case of saliva: this modification of sputum was regarded as purely mucus, possessing no albuminous matter; it was found to contain a very small proportion of solid matter in a given weight, but the quantity of saline matter was found to be considerable, when compared with the quantity of solid matter; and this saline matter the author's experiments lead him to conclude was diminished in quantity as the disease progressed. The chemical nature of sputum of the acute and chronic bronchitic character was then entered upon, and noticed as differing in certain respects from the preceding form of expectorated matter, in containing, for example, a much larger proportion of solid matter in a given weight than was found in simple pituitous expectoration, and generally a smaller proportion of saline matter; it also differed in containing, generally speaking, small quantities of soluble albumen capable of undergoing coagulation by heat.

Pneumonic expectoration was then treated of, and noticed as principally made up of a tough mucoid secretion intermixed with blood, to which last was owing its peculiar rust or brick-red colour, and also its powers of undergoing, to a certain extent, coagulation by heat when mixed with water and filtered; it was also found to differ from most other forms of expectorated matter, in containing no inconsiderable quantity of oxide of iron, derivable from the blood with which it is impregnated. Phthisical expectoration was the last form of sputum, the chemical characters of which were described. It was noticed as differing materially in different stages of the disease—in the earlier and middle stages scarcely not at all—for the most part, at least, differing from the expectoration met with in chronic or acute bronchitic affections: in the latter stages, however, not unfrequently possessing the characters of a simple collection of puriform matter, containing very large quantities both of soluble, and solid or insoluble albumen, much solid matter also in a given weight, with the ordinary saline matters found in other varieties of sputa, superadded to which was a notable proportion of oxide of iron. It was stated, that in no disease, except phthisis, did the expectoration contain so much soluble albumen capable of undergoing coagulation by heat; and also in no disease, except pneumonia, was there so large a proportion of solid matter in a given weight of the expectoration: this observation referring however to the sputum in the latter stages of phthisis, where it puts on the character of a collection of puriform matter. Allusion was then made to the fatty matter existing in expectorated fluids, which was found to be the same in quality in almost every variety of sputum, but differing in quantity, being much greater in quantity in well-marked phthisical expectoration, than in any other variety. The fatty matter was peculiar, too, from the high temperature which it required for its fusion, it being considerably higher than that necessary for the fusion of the more ordinary forms of fatty matter, and even higher than that required for cholesterine: this fatty matter was soluble in alcohol and ether, being deposited from the former when its boiling solution cooled. The author also referred to the power which a galvanic current, even of low intensity, possessed of coagulating the aqueous fluid, obtained either by digesting saliva, or any of the modifications of expectorated matter before alluded to, in water, and filtering the fluid. This coagulation was not regarded by the author as proving the presence of albumen, because, in cases in which the galvanic current effected the change in question, the most delicate re-agents with which chemists are acquainted for the detection of albumen, failed to detect the slightest trace. The author then detailed his experiments on crude and softened tuberculous matter; he submitted the former to the action of the same re-agents as he employed to re-act upon ordinary fibrine, and was led to conclude that the crude tubercle did not differ chemically from solid albumen or fibrinous matters. The mode of analysis employed in examining the crude and softened tubercle was the following—it may be observed, that the crude tubercle was examined side by side with ordinary fibrine: the crude tubercle was dissolved in a weak solution of caustic potass; a similar solution of fibrinous matter was obtained, and both submitted to the action of the same re-agents, with results as nearly similar as possible. The agents employed were the mineral acids, acetic acid, and ferrocyanide of potassium, tinct. galls, corrosive sublimate, &c. The softened tuberculous matter was first dejected in water, and then filtered; the filtered fluid, when submitted to re-agents, was found to contain soluble albumen,—that portion of the tubercle insoluble in water, was dejected in a weak alkaline fluid, by which a solution was obtained. This alkaline solution, when submitted to the necessary re-agents, indicated the existence of solid albuminous matter or fibrine; hence, the softened tubercle was regarded as analogous in its chemical characters to purulent matter. Experiments were then made on the tuberculous matter which had undergone perfect softening, and the result was, that the latter was chemically identical with pus; from which it was deduced, that fibrinous matter was, by a process of softening or fluifaction, converted into actual pus, and hence a fruitful source of the abundantly albuminous fluid found in the expectoration of patients in the latter stage of phthisical disease. The author then concluded his paper, by stating the results of a quantitative analysis of the expectoration of a marked puriform character, obtained from a patient in the last stage of phthisis. It was found to consist of: water—albuminous matter, with a little mucus—extractives, soluble in alcohol; ditto, soluble in water; fatty matter—saline matters, consisting of the alkaline chlorides, phosphates and carbonates, with earthy phosphates and oxide of iron.

Dr. Williams expressed his gratification that the labours of so expert a chemist had been directed to so important a branch of science as animal chemistry, which had been strangely neglected and almost overlooked. This he lamented the more, since the subject would promote both the theory and practice of medicine. He would, however, ask Mr. Brett if he had observed in his microscopical experiments in early phthisis, what the French had detected the sputa, a sort of downy substance, peculiar to the very early stages of this disease, and only observable through the microscope—indeed it was a microscopic phenomenon. It was a novelty to him, that tubercles by softening, as mentioned by Mr. Brett, assumed globular forms of which they were devoid at the commencement, a fact confirmed by Gmelin. This paper pleased him, for it further assured him of the truth he had taught, that softening was a modification of suppuration,—a conversion of albumen into pus. There was a total absence of organization in tubercle.—Mr. Brett had not observed the microscopical flocculent and woolly appearance, for his attention had not been so directed.—Dr. Williams explained, that it did not resemble flocculi, but was, he considered, a microscopical appearance only.—Mr. Brett had made his microscopical observations on what was obvious to the naked eye, and opaque portions of these secretions he found to be globular, not so perfect in cheesy suppurations as in those of other characters.—Mr. Williams inquired what was the opaque compound in saliva. Mr. Brett answered, it might be a modification of albumen, but he could not say exactly.—Dr. Roget remarked, that the paper made it appear that the formation of globular inexpectorated matter was spontaneous.—Dr. Williams stated, that the belief of this sort of globular formation might be supported by the circumstance of its production in serum, after it had been some time at rest, which had been shown by Dumas and Sir Everard Home.—Mr. Brett did not consider the cases parallel, since serum was highly albuminous, and this expectoration not.—Dr. Johnson thought, that independently of the chemical acumen, the paper would be of value to practitioners, on account of the physical character of expectorated matter, of which there were three modifications:—1st. Mucus, from intensity of disease in the bronchia. 2nd. Tuberculous matter softened down, and escaping into the bronchial tubes, or entirely discharging from them. 3rd. Secretion of purulent matter from excavated surfaces, scarcely distinguishable from the matter of abscess, or the after discharge from the depôt, by which he meant the matter secreted after the first contents of an abscess had been discharged.—Mr. Golding Bird, after alluding to Mr. Brett's experiments, and drawing the attention of the Section to those parts which particularly merited the attention of practitioners, offered a suggestion, in explanation of the circumstances mentioned by Mr. Brett, in which mucus, previously limpid, became opaque and deposited globular masses of albumen, after a few hours' exposure to the air; as well as of the fact alluded to by Dr. Williams, viz. the circumstance of serum of blood becoming opaque and depositing globules by exposure. The rationale of these two facts Mr. Bird conceived might depend upon the action of carbonic acid upon albumen, and upon alkaline albuminates, first pointed out by him in a series of papers published in the *Philosophical Magazine*. In the case of mucus, he believed that a notable portion of albumen was present, held in solution by carbonic acid, and that, by exposure to air or to an atmosphere of hydrogen, this solvent was abstracted in an exceedingly gradual manner, in consequence of which, albumen was deposited in the form of rounded particles. In the case of serum, the converse Mr. Bird believed to take place; for, as Mr. Brett has shown, in his paper, this fluid to contain, independent of free albumen, a small portion of albuminate of soda, it appeared reasonable to suppose, that the carbonic acid present in the atmosphere acted on, and decomposed this compound, forming carbonate of soda, and causing the deposition of albumen in a globular form. This hypothesis, moreover, serves to explain the fact of serum, previously neutral, becoming alkaline after a few hours' exposure, for the albuminate of soda previously existing, is without action on test papers, whilst the carbonate of soda, produced in the manner above mentioned, is fully capable of producing an alkaline effect on turmeric paper. This explanation is in di-

rect accordance with the experiments related in the *Philosophical Magazine*, before alluded to.

Section F.—STATISTICS.

President—Lord Sandon, M.P.
Vice-Presidents—Col. Sykes, Mr. G. R. Porter, Mr. J. Heywood.
Secretaries—Mr. W. R. Greg, Dr. W. C. Taylor, Mr. W. Langton.
Committee—Dr. Jerrard, Mr. Hovenden, Mr. Fripp, Dr. Carpenter, Mr. H. Romilly, Col. Briggs, Mr. Corrie, Mr. W. Wyse, M.P., Mr. Slaney, M.P., Mr. Urquhart, Dr. Kay, Mr. Tuffnell, Mr. Chadwick, Mr. S. J. Loyd, Mr. Simpson, Lord Nugent.

Colonel Sykes, having vacated the chair, read a summary of a paper 'On the British Collectorate of the Deccan,' prepared by the direction of the British Association, which met at Cambridge in 1833, and afterwards extended, at the request of the sectional Committee, at the Dublin meeting of the Association. (The entire paper is to be published in the Transactions of the Association. We have taken, with additions, the following abstract from the *Liverpool Standard*, as more full than the report transmitted by our own correspondent.) The four collectorates of the Deccan, within the province of Bombay, contain a population of 3,285,985 souls, and 48,987 square miles, or about 67 inhabitants to the square mile,—lying on an elevated plateau, formed by the Ghauts, and descending by a succession of steppes to the Coromandel coast. The Poonah collectorate contains 8,281 square miles, 550,313 inhabitants, 1827 towns and villages, and 114,887 houses, averaging about 4 inhabitants to a house, and 247 to a village, exclusive of the city of Poonah, which contains a population of 181,000. The rivers in the Deccan, during the monsoons, present magnificent streams of water, but, in the dry season, either a broad sandy plain, or a mere thread of water. The roads, with the exception of two great military roads, are untouched by art, and few of the rivers can boast of a bridge. With respect to Geology, there are no organic remains, and probably no country in the world in which the trap rock prevails to so great an extent. In the Deccan there are 200,000 square miles, without the intervention of any other rock whatever. This is succeeded by granite and other rocks of igneous origin, so that from the 25th degree of latitude, to Cape Comorin, including Ceylon, there are 700,000 square miles of igneous rocks and granite. The tides of the atmosphere are one of the principal features connected with the climate of the Deccan. These tides, like those of the ocean, rise and fall twice within the 24 hours, at stated periods, and with a regularity which can almost be calculated upon. During observations of four years' continuance, made with different instruments, there was no variation in the order of the rise and fall, though there was occasionally some little variation in the degree. The atmospheric tides prevail from the equator to the poles, and are very observable to the 64th degree of latitude,—the maximum being at the equator,—the minimum at the poles. They exist even in our own latitudes, with all their variations. In the Deccan, as throughout the world, the barometer ranges highest in cold weather, and diminishes during the monsoon. The temperature, at half-past nine in the morning, is the mean temperature of the year; so that a register kept at that hour, gives the mean temperature of the year. With regard to the quantity of rain, the clouds, containing the water drawn from the ocean by the action of the sun, beat against the Ghauts, and the rain which falls there is fourfold the proportion of that which falls 30 or 40 miles to the eastward. At Poonah, which is only 50 miles east, the annual fall of rain is only 25 inches, whilst in Bombay it is 100. Hail only falls at the very hottest season, with the temperature from 95 to 100. The air is perfectly clear;—suddenly the horizon is overcast, the dust is blown up in dense masses, with occasional violent claps of thunder, and showers of large hailstones. Dews are very copious,—fogs little known. The climate is very salubrious. In his (Col. Sykes's) camp, consisting of 100 persons, not a single death occurred in six years, and there was only one case of sickness which he did not cure without medical aid. In 1828, the deaths were 1.82 per cent. or one in 55 persons, not including cholera, or one in 40 including cholera, so that even in India, where this frightful disease originated, it appears to be much less serious than was supposed. Dr. Lawrence, the medical attendant at Bombay, had charge of 1000 natives for several years, and lost only 0.85 per cent. or less than one per cent. per annum.—Agriculture, though rudely carried on, is very productive; there are forty-five cultivated fruits, including six or seven species of the grape, and twenty-two wild fruits, including the mangosteen, the date, &c. &c. There are two harvests in the Deccan, one at the hot and wet season, the other at the cold or dry season, and both of distinct kinds of grain or pulse—the harvest at the wet season is principally of rice, which is chiefly produced in the hilly country. The productiveness of some of the grains is perfectly astonishing. Four species were mentioned—one producing 33 stalks, and 61,380 grains from one seed; another, 1,690; a third, 2,985; and a fourth, 1,850. One species of wheat, taken out of a field at random, and now in his possession, contained 25 stalks, and 1,450 grains, the average on tolerable land being 8 stalks to each plant. Besides this, there are corn, barley, peas, and sugar-cane. There are 46 articles of garden culture. Edible fruits are numerous, and many wild plants and flowers are used as greens. Col. Sykes stated that the natives are quite as carnivorous as the inhabitants of Europe, so far, at least, as mutton is concerned. The grasses are innumerable, some of them useful for cordage. The inhabitants make no hay, but allow the grass to remain on the ground till dry, when they cut it with sickles. There are few fens, no heaths, and no oaks, elms, or hazels. The Zoology of the Deccan exhibits specimens of all the different varieties. The wild dog is a native of the Ghauts; there are three kinds of monkeys, and two of bats. The domestic poultry of this country is supposed to have originated in India, the two species being identical. Most of the wading and swimming birds are identical with those of Europe. In noticing the fish, Col. Sykes remarked that a certain species of fresh-water fish were found in pieces of water, two thousand feet above the level of the sea, exactly resembling our own salt-water fish. With respect to population, the proportion of male to female births, which in England is 100 to 93—in the Deccan is 100 to 87; and this difference obtains, with very little variation, throughout India, modified by the singular fact exhibited in the excess of grown-up women over men. Sir Stamford Raffles, in his account of the island of Java, states that the proportion of births was 100 males to 82 females, but that the same disproportion did not exist between grown-up people. In the Deccan, the preponderance of male over female children is very strongly marked, but a greater mortality amongst the males at a subsequent period makes the females outnumber the males. The same law, therefore, appears to prevail both within and without the tropics. The average number of deaths throughout the whole collectorate was one in 37, but that was in an exceedingly bad season, when the cholera prevailed. The proportion of marriages is very nearly the same as in England and France, it being one in 125 in Poonah, one in 128 in England, and one in 130 in France. With respect to education—in one province there is only one school to 2452 inhabitants; in another, one to 4639; in a third, one to 3337. The tenures of land are exceedingly numerous, and amongst them is the freehold, which has been acknowledged by the native governments; whilst there are many descendants of those amongst whom the land was originally divided, now in actual possession. Artisans of various kinds do the work of the farmers in their respective branches, and are paid by allotments of land, and a per centage on the produce; thus, the barber shaves for his land, the tailor makes clothes for his land, &c.—which land is cultivated by them to produce food. The revenue derived by the government was 82 per cent. in the aggregate from land, and altogether averaged 8*s.* per annum for each individual. The native manufacture of silk and cotton has been almost suppressed by the machinery of England. There are few other manufacturing products of any value, and these are not produced in the Company's territories, with one or two slight exceptions. The transit duties on the conveyance of goods are exceedingly onerous, and form a great impediment to commerce.

At the conclusion of the paper, a vote of thanks was passed to Col. Sykes. In the course of a discussion which took place at a later period in the day, several questions were put to Col. Sykes, in reply to some of which he stated, that the wages of a head carpenter, as compared with the price of grain, were from 25*s.* to 30*s.* a month; a common carpenter 31*s.* to 35*s.*; a smith the same; field labourers 14*s.* a month for a man; from 7*s.* to 14*s.* for a woman, and 6*s.* for a boy, finding themselves in everything, and working from sunrise to sunset. He was afraid that the loss of the two principal manufactures was not made up or compensated by any increase of raw produce for exportation. The cultivation of various articles might be greatly increased to the advantage both of the natives and of the people of this country: for example, several kinds of oils, and many species of fibrous plants, suitable for cordage. The breadth of land under cultivation has not been increased of late years. The instruments were so rude and simple, that were not the returns naturally so great, and the cost of production so trifling, the people could not live. The condition of the labouring classes is little better than that of the people in many parts of Ireland. The system of transit duties was under the serious consideration of government. Until within the last twenty years there had been constant intestine wars, which were now put an end to under the influence of the British government.

Mr. G. R. Porter, Vice President of the Statistical Society of London, then read 'A Brief Memoir of the growth, progress, and extent of Trade between the United Kingdom and the United States of America.' He began by observing, that in these days of watchfulness and severe scrutiny into every branch of the public expenditure, it would hardly be credited that there is not a trace to be found in the Journals of the House of Commons of any account of the produce of the taxes having been called for by Parliament during the whole course of the American war,—a fact attested by the late Mr. George Rose, who having been for many years a Cabinet Minister, knew well the importance of such returns.

The British Colonies (Mr. Porter continued) which now form part of the United States of America, were, with the exception of Georgia, all founded in the seventeenth century. The date of the first settlement of each individual colony was as follows:—

Virginia	1607	Maryland	1633
New York	1614	Connecticut ..	1635
Massachusetts ..	1620	Rhode Island ..	1636
New Hampshire ..	1623	North Carolina ..	1650
New Jersey ..	1624	South Carolina ..	1670
Delaware	1627	Pennsylvania ..	1682
Maine	1630	Georgia	1733

It was not until more than a century had elapsed from the period referred to in the foregoing extract, and when they had secured their independence, that any part of the raw material employed in the cotton manufacture was received from the British plantations in America. A few bags of cotton, which arrived in 1785 and 1786, were apparently of foreign growth, and had been transmitted to America from the Spanish main. Cotton was raised in gardens in the United States before 1786; but that was the first year in which it was cultivated by planters as a crop, and 1787 was the earliest year in which any of the growth of the country was exported.

Before the separation of the British prov from the mother country, the statements which given concerning their trade exhibited that of each province separately. Attention was then directed to a table which contained the official value of imports and exports from and to each province, for the years 1701, 1710, 1720, 1730, 1740, 1750, and 1760, and thereafter for each individual year to 1783, when the independence of the United States was fully recognized. For a long period up to that event the operation of the navigation laws had given to this country a monopoly of the trade with its colonies; and Mr. Porter considered it worthy of remark, that so long as the American provinces continued thus connected with England, the increase of the commercial intercourse bore a very inadequate proportion to their increasing population. In 1749 the number of inhabitants in the provinces was stated to be 1,046,000, and the official value of exports and imports was 2,117,845*l.* Assuming that the population between 1749 and 1774 increased steadily at the rate afterwards exhibited by the census of 1790, the number of inhabitants in 1774 must have been 2,803,625. If the trade had increased in an equal ratio, the imports and exports in 1774 would have amounted to 5,676,523*l.*; whereas

the actual amount was only 3,964,288*l*., showing a deficiency of 30 per cent.

Another table exhibited the official value of our imports and exports from and to the United States collectively, in each year from 1784 to 1835.

The earliest census for the United States was taken in 1790, when the population was found to be 3,929,328. The official value of our trade with the United States in that year was 4,622,851*l*. In 1800 the population was found to have increased to 5,309,758. At the same rate of increase the trade in that year should have been 6,246,925*l*.; but as it actually amounted to 9,243,432*l*., the increase was greater than that of the population by 48 per cent. In 1810 the population was 7,239,903, and the trade 10,427,722*l*. If the proportion of 1790 had been preserved, the amount would have been 8,517,739*l*. The excess, after allowing for the increased population, was therefore 22 per cent.; but if the comparison is made with 1800, it appears that the increased trade is not quite 13 per cent., while the population was augmented at the rate of 36 per cent.; there is therefore a virtual deficiency of 23 per cent., which Mr. Porter considered ought to be ascribed to the operation of the Orders in Council issued in retaliation of the Milan and Berlin Decrees of Napoleon. Pursuing the comparison to 1820, we find that the population was then 9,638,166, showing an increase over 1810 of 33⅓ per cent.; on the other hand, there is a falling off in the official value of the trade between the two countries at the rate of 27 per cent. This circumstance Mr. Porter attributed to causes of a temporary nature, capable of easy explanation. On the renewal of the intercourse between England and America, after the peace in 1815, our merchants and manufacturers, stimulated doubly by the deficiency of British goods in the American market, and their superabundance and consequent low price at home, made such large shipments of manufactures to the United States, that a glut was there produced, and as this occurred simultaneously with a considerable derangement of the currency in the commercial cities of America, English goods were sacrificed at ruinous prices. In the meantime, the commercial distress which had visited our own country was passing away, and an effective demand for our products had arisen from other quarters, as appeared from the fact, that although the real value of British goods exported to the United States, which, on the average of the five preceding years, was near 9,000,000*l*., fell in 1820 to 3,875,286*l*., the general exports from the United Kingdom to foreign countries were greater in 1820 than they had been in the preceding year.

In 1830, the date of the last census, the population of the United States was 12,857,165, and the official value of the trade with this country 16,292,637. The increase, as compared with 1790, was 227 per cent. on the population, and 252 per cent. on the amount of trade. If the comparison is made with the remaining decennary periods, it will be found that the increase in 1830 was as follows:

		Increase per Cent. Population.	Trade.
Compared with	1800	142	76
—	1810	77½	56
—	1820	33⅓	115

The increase of population in the United States, between 1820 and 1830, was at the rate of 3⅓ per cent. per annum. If we assume that the increase has since gone forward at the rate of 3 per cent. in each year, the number of American citizens in 1835 must have been 14,784,589. The official value of their trade with this country in that year was 25,671,602*l*. A comparison of this amount with the value of the trade in the years of the different enumerations exhibits the following results:—

		Increase per Cent. Population.	Trade.
Compared with	1790	276	455
—	1800	178	177
—	1810	104	146
—	1820	53	239
—	1830	15	57

But Mr. Porter considered that it was not simply with reference to the numerical increase of the citizens of the United States that we should consider this question of the increase of our trade. During the forty-seven years that have elapsed since the first census was taken, in 1790, at least 11,000,000 of inhabitants have been added to their number, being equal to an increase of 276 per cent. But during that time we are fully warranted in believing that the wealth of the country has been augmented in a much greater proportion; and it may be fairly presumed that, but for the untoward interference of wars, and of that which is scarcely less inimical to national prosperity than war—commercial jealousy, the dealings between the two countries must have become far more considerable than they are. During the period in question, America has added materially to her means of consuming foreign products by the extent to which she has carried the cultivation of exportable products. In 1791, the whole export of cotton from the United States was under 200,000 lb.; and it is shown by accompanying tables that the average annual importation of American cotton into this country, during the last ten years, has exceeded 225,000,000 lb., the value of which cannot have been less than 7,500,000*l*. per annum. In 1836 our importation was 289,615,692 lb., which, at the average price of the year, probably produced more than 10,000,000*l*. sterling.

The intercourse between this country and the United States is important, not only to our merchants and manufacturers, but also to our ship-owners, and that in a continually augmenting degree. The tonnage of vessels which entered the ports of the United States from foreign countries, in each year from 1821 to 1836, distinguishing American and British from other shipping, was as follows:—

Years ending 30th Sept.	American.	British.	Other Foreign Vessels.	Total.	Centesimal proportion of British to American Tonnage.
1821	765,098	55,188	26,338	846,624	7.21
1822	787,961	70,669	29,872	888,502	8.97
1823	775,271	89,553	29,915	894,739	11.55
1824	850,033	67,351	35,016	952,400	7.92
1825	880,754	63,036	29,891	973,681	7.15
1826	942,206	69,295	36,359	1,047,860	7.35
1827	918,361	99,114	38,475	1,055,950	10.79
1828	868,381	104,167	46,056	1,018,604	11.99
1829	872,949	86,377	44,366	1,003,692	9.89
1830	967,227	87,231	44,669	1,099,127	9.02
1831	922,952	215,887	66,061	1,204,900	23.39
1832	949,622	288,841	104,197	1,342,660	30.41
1833	1,111,441	383,487	113,218	1,608,146	34.50
1834	1,074,670	453,495	114,557	1,642,722	42.19
1835	1,352,653	529,922	111,388	1,993,963	39.18
1836	1,255,384	547,606	132,607	1,935,597	43.62

The most important part of our trade with America consists in our exports of manufactured goods. The following table exhibits the declared value of those exports in each year from 1805 to 1836, with the exception of 1812 and 1813, the records for which two years were destroyed at the burning of the Custom House in London.†

Declared Value of British and Irish Produce and Manufactures exported from the United Kingdom to the United States of America, in each year from 1805 to 1811, and from 1814 to 1836.

Years.	Amount.	Years.	Amount.	Years.	Amount.
	£.		£.		£.
1805	11,011,409	1817	6,930,359	1827	7,018,272
1806	12,389,488	1818	9,451,009	1828	5,810,315
1807	11,846,513	1819	4,929,815	1829	4,823,415
1808	5,241,739	1820	3,875,286	1830	6,132,346
1809	7,258,500	1821	6,214,875	1831	9,053,583
1810	10,920,752	1822	6,865,262	1832	5,468,272
1811	1,841,253	1823	5,464,874	1833	7,579,699
1814	8,129	1824	6,090,394	1834	6,844,989
1815	13,255,374	1825	7,018,934	1835	10,568,455
1816	9,556,577	1826	4,659,018	1836	12,425,605

One thing which cannot fail to strike any one on inspecting this table is the large amount of our exports in the three earliest and two latest years of the series, when compared with those occurring in the intermediate years. The extent of the shipments in 1815 Mr. Porter considered as the result of the renewal of commercial intercourse after the war. The years 1805, 1806, and 1807, 1835, and 1836, followed long periods of friendly intercourse. The serious falling off that occurred in 1808 and 1809, Mr. Porter, as already stated, attributed to the effect of our celebrated Orders in Council, issued in retaliation for Napoleon's Milan and Berlin Decrees. Nearly one-third of our foreign export trade in 1805, 1806, and 1807, was carried on with the United States.

The high degree of importance to each country of the trade which it carries on with the other was shown in Tables appended to the Memoir. The proportions which that trade bears to the entire foreign trade of each country are as follows:

Centesimal Proportion which the Trade between the United Kingdom and the United States of America bore to the whole Foreign Trade of each country respectively, in each year, from 1821 to 1835.

Years.	Centesimal Proportion which the Trade with England bore to the whole Foreign Trade of the United States.	Centesimal Proportion which the Trade with the United States bore to the whole Foreign Export Trade of England.
1821	35.95	16.95
1822	38.16	18.57
1823	32.70	15.41
1824	31.75	15.86
1825	37.67	18.31
1826	29.60	14.77
1827	35.03	18.87
1828	34.75	15.78
1829	33.75	13.45
1830	33.13	16.02
1831	41.78	24.36
1832	35.99	15.00
1833	35.41	19.36
1834	39.61	16.43
1835	41.76	22.31

The proportion which our export trade with the United States bore to our whole export trade was, in—

1805	28.91
1806	30.31
1807	31.80
1836	23.28

Mr. Porter stated that in the foregoing observations all remarks upon the state of convulsion into which this most important branch of our foreign trade has lately been thrown had been avoided, partly because its occurrence is too recent to allow of a sufficiently calm estimate being made of the cause or causes which led to the catastrophe, but chiefly because it would be difficult, if not impossible, to enter upon that subject without departing from that line of strict statistical research which it is desirable to preserve in the proceedings of this Section of the British Association. In conclusion, he remarked that the shipments of British produce and manufactures, in the year 1836, amounted, according to the value declared by the shippers, to 53,368,571*l*., of which sum America took 12,425,605*l*., or 23.28 per cent. The total shipments in 1835 amounted to 47,372,270*l*., of which America took 10,568,455*l*., or 22.31 per cent., the difference between the two years being, on the total shipments, 5,996,301*l*., and on the shipments to America, 1,857,150*l*. Without admitting or denying that these figures give evidence of over-trading, he called attention to the circumstances of the two people—namely, that the means of obtaining the comforts of life are enjoyed by a larger proportion of them than is the case with any other people; that the habits and predilections of the citizens of the United States lead them to give a preference to British goods; that ours is the cheapest market in which they can procure many articles necessary to them; and that we are, out of all proportion, their best customers for the raw produce of their soil; and he asked whether, if the trade of the two countries were put upon a proper footing, and conducted upon enlightened principles, that amount of traffic *could* be considered excessive which gives annually to every citizen of the United States articles of British growth and manufacture to the value of sixteen shillings and ninepence three farthings!

A report 'On the State of Education in the Borough of Bolton in 1837,' was read by Mr. Ashworth.—The Return made to government in 1833 on the state of Education has been found very defective. In Bolton there have been no means of testing its correctness. But, if accurate, there has been a very remarkable increase in the number of scholars, being 25 per cent. more of day scholars and 40 per cent. more of Sunday scholars.

There are now 21 Sunday Schools with 9,867 scholars, or 19¾ per cent. of the population, of whom about 2000 may be estimated as being in attendance both at daily schools and Sunday schools, leaving the number of 7,867 or 15¾ per cent. of the population receiving instruction at Sunday schools only.

There are 66 day and evening schools containing 3,227 scholars or $6\frac{9}{20}$ per cent.

† This omission is less to be regretted, because of the unfortunate state of hostility into which the two countries were plunged during those years.

Total number of scholars 11,094, or about 22⅕ per cent. of the present population, estimated at about 50,000.

Children equal in number to 20 per cent. of the population are not in attendance at any school whatever.

In the Sunday schools were found—

2,014 scholars in 4 schools connected with the Church Establishment.
1,085 scholars in 1 Roman Catholic school.
6,763 scholars in 16 schools belonging to various classes of dissenters.

In Bolton there are 5 charity schools with 692 scholars, including the two infant schools. There is also a grammar school, whose scholars have been entered at 120, being the number reported to government, the master having declined to give our agent any information on the subject. The income was stated to the committee to be 450*l.*

Of superior schools for the children of persons in good circumstances there appear to be 17 with 721 scholars.

Of common boys' schools there are	15	with	851 scholars.
Of common girls' schools	5	—	209 —
Of Dame schools	23	—	634 —

—944 being boys and 750 girls, all the boys' schools containing some girls, and *vice versâ.*

Extracts from the reporter's notes will show that even this limited education is of a very inferior quality:—

"I find that in many of the schools there are, in many cases, from twenty to a hundred scholars, crammed into a dirty room or cellar, without air or ventilation, the effluvia from whose breath and clothes is exceedingly offensive, and must be very injurious to the children's health. In most, too, ordinary household occupations have been carried on by the old women along with the teaching of the scholars. In some instances, the neighbours were sitting over the fire in the school, smoking their pipes to chat and gossip.

"A good deal of the bad morals, bad manners, and absurd prejudices, which we find amongst our population, are perpetuated by the example of the teachers and their associates. It was sometimes difficult to get questions answered. To the inquiry as to the method in teaching arithmetic several of them replied, '*Why th' graidley owd-fashioned road.*'

"One of the masters, whose head was bound up with a dirty rag, and whose house, in a back street, seemed never to have been cleaned, told me, in answer to the question whether he was educated for the employment, that he was so educated, adding, '*My feyther larnt eight parts of speech besides English, and parson Fonds toud him tin he could teych him no feer.*' Upon my remarking that I supposed he would also have been liberally educated, he said, '*Oh yes, I larnt accidents and grammar.*' His occupation he said had been that of a navigator, or, as he explained the term, 'he had worked at making lodges and reservoirs.' Necessity, not fitness, seems in almost every instance to have been the cause of the teacher's adopting this employment, as is evident by a perusal of the answers which they have given on being asked what inducement led them to undertake the profession of a school-master. 'Old age, and to get a living,'—'My husband left me with four small children, and I undertook it to get a living,'—'My husband could not keep me, so I took this because I could get nothing else.' One man gave as his reason that he had lost his left arm, and a woman that she had lamed her foot. Another old woman said she kept a Dame-school because '*she geet* poor and was a widow.'"

'An Abstract of the Report made by the Regents of the University of the State of New York, on Education,' was then read by Dr. W. C. Taylor.—The Regents are required by law to make an annual report to the legislature on the state of education in the several colleges and academies subject to their visitation. Dr. Taylor observed, that accurate forms of returns are sent to these Institutions, and though they have not all been filled so completely as could be desired, they enable us to form a pretty accurate view of the present condition of education in that State, and its tendency to progressive improvement, especially as regards the instruction of the higher and middle classes. Under the direction of the Regents, departments have been added to the collegiate systems, for the instruction of common school teachers. The whole number of students in these departments is 228, an increase of 110 in the year. The inadequate compensation paid to teachers has hitherto deterred many from entering the profession; but since these departments were instituted, the standard of common schools has been greatly raised, and ordinary members of them obtain from 50 to 75 per cent. more than was paid to the best teachers six years ago. The average was below 12 dollars per month, but some of the best instructed teachers now obtain more than 18 dollars. The whole number of students belonging to academies under the control of the University, is 6,056.

The number of students stated by the trustees, to have pursued classical studies, or the higher branches of English education, or both, for four months of said year, is 4,590.

The amount of money received by the Regents from the income of the Literature Fund, is 12,000 dollars.

Value of land and buildings belonging to academies	D. 460,503
Value of other real estate	44,222
Value of philosophical apparatus and library	32,927
Value of other personal property	154,712
Number of books in libraries of academies	10,324
Tuition money for year ending March 1, 1837	6,431
Interest on Income from permanent funds	12,898
Number of teachers	261
Salary of teachers	93,062

whence it follows, that the average income of each teacher is about 356½ dollars.

The number of academies reporting to the University, including male and female seminaries, is 69; the subjects of study are thus enumerated:—

Arithmetic is taught in all the academies.
Algebra, ditto.
Architecture in 1
Astronomy in 56
Botany in 30
Book-keeping in 45
Biblical Antiquities in 2
Biography in 1
Chemistry in all.
Composition as often as once a fortnight in all.
Conic Sections in 4
Constitution of the United States in 15
Ditto of the State of New York in 5
Elements of Criticism in 8
Declamation in all but the female schools.
Drawing in 14
Dialling in 1
English Grammar in all.
Evidences of Christianity in 7
Embroidery in 1 ladies' institution.
Civil Engineering in 4
Extemporaneous Speaking in 3
French Language in 51
Geography in all.
Physical Geography in 4
Geology in 5
Plane Geometry in all.
Analytic Geometry in 4
German Language in 1
General History in all.
History of United States in 50
History of New York in 3
Hebrew Language in 4
Greek Language in all but 5
Grecian Antiquities in 4
Italian Language in 3
Latin in all but 3
Criminal and Mercantile Law in 2
Logic in 28
Levelling in 2
Logarithms in 2
Instrumental Music in 6
Vocal and Instrumental in 5
Vocal alone in 2
Mapping in 5
Mensuration in 12
Mineralogy in 4
Mythology in 3
Natural History in 17
Navigation in 9
Nautical Astronomy in 1
Natural Theology in 13
Orthography in all.
Natural Philosophy in all.
Moral Philosophy in 37
Intellectual Philosophy in 39
Penmanship in all.
Political Economy in 1
Painting in 6
Perspective in 1
Physiology in 3
English Pronunciation in 1
Reading in all.
Rhetoric in all but 6
Roman Antiquities in 10
Stenography in 2
Statistics in 1
Surveying in 49
Spanish Language in 5
Trigonometry in 23
Topography in 1
Technology in 4
Principles of Teaching in 14

It would be very desirable to obtain similar returns respecting the branches of study open to youth of the middle and upper classes of England, but such information could not be obtained by individual enterprise; the person making the inquiries would probably receive, in most instances, a sharp rebuke for his curiosity, and would assuredly be refused the means of verifying the accuracy of his returns. Were such an inquiry instituted by authority, it would probably be found a valuable addition to the American form to have columns for the number of students in each branch, and the length of time devoted to its study.

In the American Report there is no notice of any provision for physical training: should similar inquiries be made here, it would be well to have returns of the size of play grounds, and the nature of the gymnastic exercises.

The American Report contains the names of the various books used in the academies; the only remark that need be made on this return is, that a preference seems generally given to the works of American authors.

Meteorological observations, on the plan recommended by Sir John Herschel, have been recorded at 53 academies; some of these are open to suspicion of inaccuracy; but the Regents have a plan under their consideration for rendering them more perfect and complete in future. In this year's reports the latitude and longitude of the different academies is given, their elevation above the level of the sea, and such topographical remarks as best elucidate the salubrity of their situation.

The charges to students vary considerably in the different academies. Elementary instruction is generally rated from 2½ to 4 dollars per quarter, and the higher instruction varies from 5 to 12 dollars. The Albany Female Academy, which appears to be of the highest rank, charges 225 dollars annually for board and tuition in all its branches. In some of the academies emulation has been discouraged as a dangerous stimulant to action, in others it is still deemed advisable to offer premiums for exertion; both parties affirm that experience has proved the correctness of their views; but neither have supported their assertions by a reference to appreciable facts.

These are the chief statistical results of the Report; the remarks on the effect of moral discipline, and on the science of education, though very valuable, could not be noticed in this Section.

It was early in the day agreed that discussions should be adjourned until all the papers were read; and now, in consequence of the great variety of subjects, they were merely conversational. Mr. Slaney proposed that Mr. Porter's paper should be published in a cheap form, and extensively circulated.

SECTION G.—MECHANICAL SCIENCE.

President—Rev. T. ROBINSON, D.D. F.R.S.,
Vice Presidents—DIONYSIUS LARDNER, L.L.D. F.R.S., Professor WHEATSTONE, London, Professor WILLIS, Cambridge.
Secretaries—WILLIAM RITCHIE, L.L.D. F.R.S., THOMAS WEBSTER, Sec. I.C.E., CHARLES VIGNOLLES, C.E.
Committee—John Scott Russell, F.R.S.E., John Taylor, F.R.S., John Robison, F.R.S.E., Davies Gilbert, F.R.S., Dr. Traill, F.R.S.E., George Rennie, F.R.S., Rev. John Blackburne, Pres. Sheffield Phil. Soc.—*Associates:* Messrs. John Isaac Hawkins, C.E., William Fairburn, Peter Clare, George Forrester, Edmund Bury, William Fawcett, Henry Booth, Hardman Earle, Joseph Locke, David Hodgson.

The Section met at eleven o'clock, the attendance not being very numerous at first, but gradually increasing, until at the close of the day the room was completely crowded. The business was commenced by Dr. Lardner, who, after apologising to the Section for his inadequacy to do justice to a paper only just put into his hands, and declaring himself not responsible for the opinions given in it, proceeded to read Mr. Remington's paper 'On the Railway Balance Lock,' (Mr. R. himself being absent).

This balance lock is designed to raise or lower a train of carriages by a horizontal motion. The trains are proposed to be elevated on a stage of wood or iron raised or lowered by wheels and axles upon train plates or rails laid in a series of inclined planes; the construction of which was explained by a diagram. The stages are proposed to balance each other as far as their own weight will suffice, and the power required is to be supplied by a stationary steam-engine. The author believed that this system would be found superior both in cheapness and dispatch to any system of inclined planes; he described it as an attempt to convert a railway into a series of level planes broken by steps. Its general principle was to lift the trains from a lower to a higher level, by a single line of rails placed on a platform which was to be raised perpendicularly between the walls of the lock. At the sides of the platform are wheels or rollers, which are intended to play in diagonal grooves in the walls, the platform being connected by wrought iron rods with a superior bar; there is a similar platform on the other side of the engine; and the trains are raised by a series of levels.

Mr. Williams's paper on the Treffos Pump was next read. Great difficulties arise in the case of the common pump, wherever there is a long column of water between it and the well. It is proposed by this pump to keep up a continuous motion in the column, however long, thus permitting the pump to be placed in the most convenient position; it was said to save the power by which a long column of water is set in motion afresh at each stroke of the pump, and to save expense in the dimensions of the supply pipe by keeping up the continuity of the action, a purpose hitherto attained imperfectly by the use of two or more cylinders acting in succes-

sion, which Mr. Williams thought complicated and inapplicable to the purposes of the common house pump. Mr. Williams places an air-tight vessel or chamber which he calls a "treffos," adjacent to and of somewhat larger dimensions than the cylinder of a common forcing pump, the bottom of each being connected by a suction pipe. The treffos is to be quite filled in the first instance with water through an aperture in the top, and to be completely closed when the pump is set in motion. As the piston ascends, the water below rises in the cylinder and falls in the treffos, until the water is on a level in each, there being no atmospheric pressure in either.

Mr. Evans thought the proposed plan rather disadvantageous than otherwise. He and Mr. Addams considered that it was only an enlargement of the common pump, and subject to the same defects, the friction being greatly increased moreover, by the use of a pipe of so small dimensions as that proposed, 3-4ths of an inch. The difference of friction was very perceptible between pipes of 1¼ inch and 1½ inch diameter; it must, therefore, be very greatly increased indeed in a pipe of 3-4ths of an inch; while the employment of the second valve was also objectionable. If three valves were to be employed, the same effect might be obtained without the large chamber.—Mr. Knight inquired whether the pump could be removed to a distance from the well, a case in which the common air vessel could not be used. The Secretary explained that such was Mr. Williams's assertion, the advantage consisting also in equalizing the current, and in the *certainty* of obtaining a cylinder full at each stroke. The object was to render the piston independent of the supply pipe, but dependent on the larger vessel. It was answered that for the certainty asserted to be given, the action of the piston must still be depended upon.

The Section then proceeded to the consideration of Mr. Henwood's paper 'On the expansive Action of Steam in the Cornish Mine Engines.' Mr. Henwood exhibited tables and diagrams illustrative of the various elements which influence the expansion of steam in the cylinders of some of the large pumping engines in these mines. The diagrams pointed to differences in the early part of the stroke depending on the quantities and pressures of the steam in the boilers, on the dimensions of the valves, and on the load of the engines; in the middle of the working stroke of all of them parabolic curves described the pressure of the steam at successive instants, and at the end of the return stroke another parabola indicated the saving obtained by expansive working. In three engines at Huel Towan, East Crinnis, and Binner Downs, the relative duties performed by one bushel of coal were respectively 86, 73, and 73 millions of pounds lifted one foot high, and 1085, 870, and 1006 *tons* lifted the same space, for *one farthing of expense*.

A long and desultory discussion followed. Mr. Henwood stated it as his belief that in instances where results considerably differing from those given by him had been obtained, as where the *duty* had been reported to be 120 millions instead of 80; some deception had been resorted to, probably by the workmen, in support of the qualities of their favourite engines, such as putting in a very large quantity of coals before the engine commenced, and then removing it prior to its being inspected. In his own experiments he had never known the duty to be more than 91 millions, the lowest being 70 millions. In answer to a question, put by the President, as to how he determined the quantity of water raised in determining the duty, Mr. H. stated the loss of water to be about 1-10th. His experiments, in answer to a question put by Mr. Ilam, he stated to be of from twenty to thirty hours.—Dr. Lardner said the question before the Section was of great importance in a general point of view, being in fact this: "What amount of mechanical virtue resides in a quantity of coals of given quality?" Mr. Taylor had last year informed them that 125 millions of pounds were raised a foot high by the combustion of a bushel of coals, though it had previously been much doubted whether 110 millions could be so raised. Mr. Ilam replied that in no instance, at least from a long experiment, had the duty much exceeded 90 millions. Mr. Galloway had, from an experiment of twelve hours obtained a duty of 125 millions, where it had formerly been estimated at 80 millions, but he was probably deceived in the way described above. In another instance 90 or 91 millions were found to be the correct duty where 127 millions had been reported to be so.—In answer to a question from Mr. Guest, Mr. Henwood stated that the engines did best duty at a velocity of from 5 to 7 strokes a minute, the length of the stroke being ten feet: he now confined his observations to pumping engines, without reference to winding or stamping engines. Some single engines working expansively did better duty than double engines working not expansively, the duty of the former being 30 or 40 millions of pounds, that of the latter not more than from 10 to 20 millions. The variation of the strokes was not more than two or three inches in ten feet. At the end of the stroke a bell is touched by the engine, and is not left untouched more than once in a thousand times. Mr. Evans had made an experiment on this subject by suspending a piece of jack-chain corresponding to the length of the stroke, and had found that in the best engines there was an intermission sometimes every fifth or sixth time, sometimes every eighth or ninth; the variation being sometimes scarcely perceptible, sometimes half an inch, sometimes 5-8ths of an inch. Mr. Henwood mentioned that the engine *must* reach within three or four inches.—Dr. Lardner inquired whether the indicator was used to estimate the vacuum as well as the steam. Mr. Henwood said they applied it to steam only, as being what was to them most important.—Mr. Webster knew an instance where a rotatory engine altered did rather more duty than a single pumping engine. The discussion ended by Dr. Lardner's declaring his intention to propose a recommendation that measures be taken to determine the duty of double engines expansive and not expansive, acting as used in manufactures; as also the pressure of the steam, the quantity of the water, and the size of the valves.

The attention of the Section was next occupied by Mr. Russell, of Edinburgh, 'On the Mechanism of Waves in reference to Steam Navigation.' Mr. Russell had, at previous meetings of the British Association, given an account of his investigations on the resistance of fluids to the motion of vessels, and ascertained the law of interference of the wave in modifying the nature and amount of that resistance. Since the last meeting of the Association he had extended his observations to a variety of subjects of practical importance, and amongst others to the improvement of the navigation of such rivers as the Thames and the Clyde, in which steam navigation was extensively employed. In these rivers it was found that steam navigation was conducted under very great disadvantages when compared with the open sea. Mr. Russell had discovered that in shallow water one great impediment to high velocities was the generation of, what he termed, the great wave of translation of the displaced fluid,—not undulation of fluid, but translation of one part of the fluid, reaching to the whole depth with equal velocity. When the vessel is propelled, the water heaped on its side generates this great anterior wave of translation, which increases as the velocity increases; the section of displacement of water is increased in the ratio of the sine of inclination. In one instance where the depth was five feet, the anterior wave was three feet above the level of the water, so that the bow was buried in it, and when the vessel stopped the wave moved at eight miles an hour, and though the vessel drew but 20 inches water, her helm was knocked off. This anterior wave moves with a given velocity proportionate to the depth of the fluid, equal, in fact, to the fall of a heavy body through half the fluid. In some cases, the boat being stopped, Mr. Russell had followed the wave for a mile, and found it advance at the same rate. The object then would be to make the centre of the vessel coincide as much as possible with the centre of the wave, thereby diminishing the anterior wave and diminishing the resistance. This wave is at present generated to so enormous an extent that in one case the waves extended to a considerable depth for a mile and a quarter, the depth of the river being increased 1½ foot in a channel of 500 yards. In six or seven feet water the immersion would be three feet more at stem than when the boat was at rest, the progress being doubly impeded by the anterior wave and by the stern depression. The question then was to what was the wave due? and how was it to be got rid of? In general, the greater the difference between the velocity of the vessel and that of the wave the more the impediment was diminished. The increase of the velocity of the anterior wave relieves the vessel, and this is obtained not by widening but by deepening the channel, while at the same time the velocity of the stern wave is increased, so as to come forward to the centre of the vessel. In one instance a vessel moved at the rate of 4 miles with 22 strokes a minute, at 6 miles with 35 strokes, and at 5½ miles with from 60 to 70 strokes. The next great impediment to steam navigation consisted in the formation of lateral currents on the side of the vessel, which, having the same direction with the motion of the paddles, had the effect of diminishing the relative difference of the velocity of the paddles and of the fluid, and thus diminished the propelling power of the paddles, the engine being obliged to make an additional number of strokes. The third evil arose from the stern or posterior wave or surge, by which great injury was done to the banks of the river and to the smaller vessels navigating it. At an increased velocity this wave rises in a cycloidal form into a breaking surface. The remedy for these evils was to be found, not in widening the river, as generally supposed, nor in giving gradual or gentle slopes to the sides of the channel, but in deepening the river and rendering its sides as nearly vertical as possible, by which the impediments were diminished to a very great amount. Mr. Russell had made experiments with different forms of channels, as:—

The general result was, that in a rectangular channel the velocity was that due to the fall through half the depth of the channel. Thus the velocity of a wave of one foot was three miles an hour, of one of four feet eight miles, of one of fifteen feet fifteen miles. In all cases the rectangular channel was found to be the preferable one. Such a channel would generally be the most expensive, but sometimes, where, as on the Thames, the land adjoining was of high value, and gentle slopes to the banks were therefore not attainable, the rectangular would be the cheaper form.

The next wave generated was what Mr. Russell termed the wave "of unequal displacement," arising solely, it was found, from the form of the vessel. This wave was seen diverging on both sides of the vessel, from the bow towards the stern, arranged in two straight lines extending to a great distance behind it. This wave might be greatly diminished, and sometimes almost entirely removed, by giving the lines of displacement a slight concavity towards the stern, the vessel being sharpened out. When the vessel does not raise the water in given uniform progression, but is so bluff that certain points displace more than others, an anterior wave is formed of excessive displacement, the injury done by which is only inferior to that of the stern surge.

Mr. Fairburn, of Manchester, stated, in reply to a question put by Dr. Lardner, that the results of his experiments corresponded with those obtained by Mr. Russell, and mentioned one instance where, at a velocity of seven miles an hour, the channel being five feet deep, the stern was dragging on the ground.—Mr. Herapath inquired what posterior form of vessel Mr. Russell had found the best. Mr. Russell stated that on this point the result of his experiments indicated a form very different from that approved of by naval officers in general. They preferred a form bluff in front and tapering towards the stern. Mr. Russell's experiments went to show that this should just be reversed, and he had made sixteen of them at different velocities, from three to fifteen miles an hour. In the navigation of the Clyde, the progress of the formation of vessels had been in accordance with this opinion. At first they were built very bluff, with their maximum breadth at a distance from the stern of 1-3rd of the whole length; thus a wave of excessive displacement was generated, going off at right angles, and making a break more than was necessary to allow the stern to pass through. Now the best vessels were built with full sterns and narrow stems, with their maximum breadth at midships. For working well, however, a very deep keel was, he knew, necessary to give the helm full effect. In answer to the question whether these experiments might be made with model vessels on a small scale,

Mr. Russell said that experiments with models were generally very fallacious and complicated, and that his had been made with vessels from 75 to 100 feet long. When asked whether they were made with or against tide, he replied that the existence of a previous current modified the velocity of the wave, which was to be measured by the velocity of the water, not by the land.—Mr. Wenfall observed that Mr. Russell's statements were corroborated by an observation of his own, that in an instance where the tide rose thirty-six feet, the effect of the lateral waves had been to form a rectangular excavation to four or five feet.

Mr. Kingsley exhibited a model of a perspective drawing board. Immediately to the left of the draughtsman a groove runs the breadth of the board in which the short arm of a T rule slides to draw the horizontal lines on the paper, which is wafered near to this groove. By reversing the rule, the short arm moving along the nearer edge of the board, the perpendicular lines are produced. By this device the parallel rule is superseded. A protractor is embedded in the board; the required angle is obtained by a T rule with a moveable arm, and is transferred to the paper after the foregoing manner: the sides of the board are provided with graduated scales. Drawing boards in common use are small where the vanishing points are at a great distance from the paper, and secondary scales are resorted to in order to bring them within range of the representation. The lines generated from these cannot be so accurate as when the primary ones are direct from the real vanishing points. This is provided for in the board we are describing, which may be of any dimensions, moveable, or fixed as a table in the study of the engineer. The rules are very broad to prevent their springing or slipping; the under sides in all are slightly excavated, the grooves being as broad as the wood will admit of.

The members of the Association dined together at Lucas's Rooms. The arrangements were far better than those at the Ordinary in Bristol, and there were, consequently, no complaints of want of accommodation. The usual toasts were given, and a few brief speeches made, after which the company adjourned to the Amphitheatre.

EVENING MEETING.

The Marquis of Northampton, on taking the chair, said, that this was the second meeting of the Association in which science had been brought to bear on a commercial population; and that the town which was thus distinguished, was one in which commerce had most experienced the results of science, for the latter had given to its vehicles on land and to its vessels by sea the velocity of the eagle. The Association held its meeting in Liverpool at a time when men's minds were excited; but the Association was neither conservative nor reforming,—neither bound up with Episcopalians nor Dissenters. It taught but one great lesson—a lesson bequeathed to us as a legacy by the author of our faith, "Love one another." The advantages arising from such meetings might be regarded as three-fold: first, the improvement, by the stimulus of excitement arising from the aggregation of persons interested in the advancement of human intelligence; secondly, the true increase of reverence for the Deity, by exemplifications of his attributes as a moral governor of the Universe as well as its creator; and, thirdly, by its effects in spreading "peace upon earth, good-will towards men." It had been said that philosophers made science an idol, and raised it above the revelation which the Deity had made to his creatures: but he was fully persuaded that science was only the high priest in the great temple of Nature, pointing out both the "how" and the "why" the great Author of existence was to be worshipped, and receiving revelation to supply deficiencies which science itself pointed out. His Lordship alluded to the diversity of feeling which existed on religious and political subjects, and expressed a hope that science would soften down asperities, melt and solve the harsher feelings, and combine all in endeavours to extend the honour of God and the good of man. They were met on neutral ground, and he was sure no angry feelings would disturb the harmony of investigations intended for the universal benefit. After passing a very high eulogium on his successor, and warmly thanking the Association for the kindness shown him by the members, his Lordship resigned the chair to the Earl of Burlington.

The Earl of Burlington, on taking the chair, acknowledged the high honour conferred on him of being chosen to preside over the aggregated science of the British Empire, but great as was that honour, he felt that the difficulty and the anxiety of such a position were still greater. It was, however, a relief to him, that the Association met in a commercial city, one in which the practical and beneficial results of science in actual life and business had been tested by experience. Though surrounded by many distinguished cultivators of science, men who enjoyed not only British, but European, he might almost say Universal fame, he could not avoid lamenting the vacancies in the circle—the absence of those who had given life, vigour, and lustre to previous meetings of the Association. Mr. Vernon Harcourt was, for the first time, absent; he who had watched over the cradle of the Institution, fostered its growth, and most efficiently aided in the developement of its strength. The loss of such a man was to be deplored, and it was with grief that his Lordship announced the termination of his official connexion with the Association; but he trusted that Mr. Vernon Harcourt would yet, as a private individual, witness the growing prosperity of an Institution for whose advancement he had so long and so strenuously laboured. The venerable Dr. Dalton was also absent; the infirmities of increasing age had compelled him to abstain from meeting those who delighted to honour the philosopher whose life had been devoted to science, and whose reward had come late: but it was a reward whose justice all acknowledged, and the honours conferred on Dr. Dalton were as gratifying to the public as to himself. His Lordship then observed, that when he looked on those who surrounded him, and saw among them Professor Whewell, a native and an ornament of the county palatine of Lancaster, he could not avoid declaring, that to that gentleman's direction, conversation, and advice, he stood indebted for whatever taste he had for science and scientific pursuits. He then referred to the advantages which general science had derived from the labours of the Association (these will be found fully detailed in Dr. Traill's report), and concluded by saying, that though the dark fields of nature were far more extensive than those which the human mind had examined, traversed, and surveyed, still the powerful minds engaged in investigation, were daily opening untravelled paths for the progress of intelligence, giving to science new stores, and to exertion fresh incentives. His Lordship then called on Dr. Traill to read the Report.

Dr. Traill, the Secretary, then read the following

REPORT.

The duty of addressing the British Association, on this occasion, was originally confided to one admirably qualified to do justice to the task; and few persons have more cause to lament the circumstances which deprive us of the services of that gentleman, than the individual who now addresses you. To those who know me only as connected with my present domicile, my position at this Meeting may appear unwarrantable or presumptuous. I can only plead, that though highly honoured by the office, it certainly was neither expected nor solicited by me; and that, unless twenty-eight years' residence in this place, and the existence of numerous and valued local attachments, may be considered as conferring the privilege, I fear I can advance few claims to be received as one of the Secretaries for Liverpool.

The objects and nature of the British Association for the Advancement of Science have been so eloquently handled by my predecessors, that to some members the subject may appear to be exhausted; but, as the Association is necessarily a very fluctuating body—as many have now joined it for the first time—and, as there still seems to be considerable misapprehension in the public mind regarding its objects and utility, a few remarks on the purposes it is intended to accomplish may not be altogether misplaced.

The British Association was undoubtedly suggested by the successful efforts of the philosophers of Germany, within the last few years. The obstacles to the free intercourse between scientific men, in that part of Europe, had always been felt as a great bar to the advance of science. Under such a system, those who, in sequestered regions, had long pursued laborious investigations, had often the mortification to discover that they were following paths trodden by others, or in which they had been completely anticipated by more fortunate inquirers. To obviate such grave inconveniences, and to promote social intercourse among men of science, scattered over wide regions, separated by physical and political obstacles, though connected by one common tongue, were the objects of that great Continental Association; and that these have been, to a considerable extent, realized by the annual assemblages of the illustrious sons of Germany, is generally admitted.

In our more united and highly-favoured land, the facilities of intercourse between its most distant points, the less isolated position of our philosophers, undoubtedly render the progress of science less dependent on such general associations of its cultivators than in Germany: yet it has never been doubted, that the personal intercourse of men engaged in similar pursuits is favourable to the progress of philosophical investigations, by the direct assistance derived from the experience and suggestions of others, and by fostering that generous emulation in the search after truth which imparts a wholesome stimulus to mental exertion, while it tends to smooth the asperities occasionally engendered by controversy, even in the abstract sciences. Men accustomed to meet and act together for one great end, naturally and insensibly imbibe the social spirit—scientific jealousy and personal rivalry are softened by mutual approximations; and individuals composing the Association, like members of the same family, learn to temper the pursuit of personal distribution by an honest exultation in whatever redounds to the honour and celebrity of the body to which they belong.

These advantages the British Association shares in common with many other societies; but it possesses characteristics peculiarly its own. It can scarcely reckon a period of infancy;—it sprung at once from the conception of its founders, like Pallas from the head of Jove, in the perfection of youthful vigour—secure in the panoply of rectitude of purpose against open or secret hostility. It quickly numbered in its ranks the *élite* of the philosophy of the United Kingdom; and, strengthened by the accession of foreign associates of distinguished reputation, it has extended its views beyond its original horizon, and has attained a colossal magnitude that distinguishes it above every other scientific association in the British empire.

This Institution ought not to be considered as the rival of any of the previously existing philosophical establishments which give lustre to these kingdoms. It, indeed, receives communications on every branch of scientific inquiry, but it professes to publish none of the numerous contributions which have given rise to the interesting and animated discussions in its different Sections: a short abstract of these papers is all that it attempts to promulgate; but the distinguishing features of its publications are those invaluable Reports on the progress of science which the Association has confided to some of its members, especially selected for that important duty.

The advantages thus conferred on general science will be best appreciated by persons whose studies are directed to any of the subjects discussed in the Reports, and who have once felt the want of an accurate analysis of what had been recently added to our previous stock of knowledge; but it would be impossible to calculate in how many instances those abstracts of precise and useful information have saved the time, and abridged the labour, of the retired student, in tracks already explored by other philosophers. Another peculiarity in the publications of the Association consists in the circulation of *desiderata* in different branches of science. The attention of their cultivators, thus drawn to the principal deficiencies in each, has already filled up various chasms in the paths of intellectual exertion, and stimulated to inquiries that cannot fail to lead to important results.

It soon became apparent that the British Association must exercise a powerful influence on the general diffusion of science, and could undertake, or materially promote, investigations to which individual research, and unaided exertion, are utterly inadequate. Its annual migrations, and the comparative ease of admission into its ranks, have unquestionably increased the taste for scientific disquisition; and, although

it would be absurd to suppose that all who seek for enrolment in the Association are destined to extend the boundaries of science, who can believe that familiarizing large masses of the community with such investigations, and exhibiting how the highest branches of philosophy may be made available to the purposes of life, will fail to promote the avowed purpose of our meetings? Who will venture to deny, that the contemplation of the galaxy of illustrious men, mustered on occasions similar to the present, has often proved the first impulse to the secret aspirant after honourable distinction—has afforded the *Promethean spark*, that kindled the sacred flame in the breast of slumbering genius?

The Association has not failed to use its influence in stimulating our rulers to aid the progress of science. At its instigation, the British government has taken up the task of the reduction of the enormous mass of observations on the heavenly bodies, accumulated since 1750, at the Greenwich Observatory—which, though collected at a great expense to the nation, and by the exertion of consummate skill in the observers—which, though pronounced by the highest authorities in Europe to be of the utmost moment to the future progress of astronomy,—have been permitted to remain a rich, but unexplored, mine of facts. The voice of our petition has been heard—the work has been auspiciously begun—and 500*l.* have been assigned by the Treasury for the commencement of this great national work.

The subject of the Tides, so strangely neglected in this great maritime country, from the period of the promulgation of the Newtonian Theory to our own times, has engaged the attention of the Association from its commencement. The advances which have recently been made on this subject, and which have greatly altered the aspect of that branch of science, had chiefly for their original basis the very valuable tide observations made in this port, many years ago, by Mr. Hutchinson, a dock-master, embracing an interval of above thirty years. The originals are preserved in the Lyceum Library of Liverpool; and, by the liberality of the proprietors, have been confided to the hands of Mr. Lubbock, under whose direction the discussion of them, ordered by the Association, has thrown a new light on the laws of Tidal phenomena.

Since that time, the earnest representations of a distinguished Associate, whom this county claims as a native, have given rise to a most important set of observations on the tides. Mr. Whewell, by personal application to the chief of the coast-guard service, and solicitation to the Admiralty, has procured the completion of a continuous series of observations, at upwards of 500 stations, along the coasts of Great Britain and Ireland. They were continued for a fortnight in June, 1834, and again in June, 1835, when they were extended from the mouths of the Missisippi to the northern extremity of Europe. These observations have been *discussed* at the expense of the Admiralty; but, as I shall presently mention, the Association has voted a large sum to be applied by Mr. Lubbock to the same subject.

These discussions have, within the last few years, led to very curious results: for instance, to the fact of *the rise of the mean level of the tides, in proportion to the fall of the barometer, and the existence of a diurnal tide*—i. e. the difference between the morning and evening tides of the same day. This diurnal tide, it may be interesting for the inhabitants of Liverpool to know, was first marked in the tide tables constructed by a young ingenious townsman, Mr. Bywater, jun., who has, unfortunately for science, died since the last Meeting of the Association.

The importance of the subject, and the success already obtained, have encouraged the Association to direct the discussion of the Tidal observations recorded at the port of Bristol, and at the London Docks; and to supply the means of defraying the necessary expense.

The influence of researches on tidal waters to navigation and to commerce are too obvious to require illustration: but perhaps it may not be unsuitable, in this place, to refer to the deductions of our eminent associate, Captain Denham, on the capability of the Mersey "to command a navigable avenue to the ocean, so long as its guardians preserve the high-water boundaries from artificial contraction." It may also be stated, that in our Transactions, this gentleman has recorded his most important general inference (drawn from a connected series of observations on the tides, which the liberality of the Dock Trustees of Liverpool enabled him to carry on)—*that there is one invariable mean height, common to neap and spring tides*—THE HALF TIDE MARK—a point from which engineers, geologists, and navigators will henceforward commence their calculations, and adjust their standards of comparison.

The Association made application soon after the meeting at Edinburgh for the resumption of the Trigonometrical Survey of Scotland; a work imperiously demanded by the imperfect state of our best maps and charts of that part of the island, either for the purposes of geology or navigation. It is needless to give further proof, than that parts of several of the large islands at the mouth of the Clyde are laid down several miles out of their true position. The magnificent scale on which the survey of Ireland is now carrying on, emboldened various scientific societies of Scotland this year to memorialize the government on the subject. I am happy to add, that the applications have been successful, and the triangulation of Scotland will recommence early in 1838.

The British Association may also boast, that at its instigation our illustrious associate, Arago, moved the Bureau des Longitudes to solicit from the French government the publication of the series of observations on the tides at Brest, and a reduction of the astronomical observations made at the École Militaire. The Brest observations have been printed, and a copy of the valuable documents put in the hands of one well able to appreciate them.

At the Dublin meeting, a committee was appointed for representing to our own government two objects important to science; which can only be accomplished in a satisfactory manner by the rulers of a powerful nation, or by an union of governments in the cause of philosophy. The first related to the establishment of Magnetical and Meteorological Observatories, in different parts of the earth, furnished with proper instruments, and in which the observations should be conducted on acknowledged and uniform principles. The extent, and the variety of climate of the British possessions, indicate them as favourable points for such establishments, which have already been commenced in France and its dependencies, and may hereafter, by the co-operation of the several governments of Europe, and of our transatlantic brethren, be extended over a large portion of the civilized world. The second suggestion was the importance of an Antarctic Expedition, for prosecuting discoveries and observations in Geography, Hydrography, Natural History, and, above all, Magnetism, with a view to determine the positive southern magnetic pole or poles, and the direction and intensity of the magnetic force in antarctic regions. The East India Company was likewise to be requested to favour the same objects, especially at their establishment at Madras.

The General Committee some time ago made application to the authorities, both in France and this country, respecting some mode of instituting a reciprocal protection to literary property. Might I venture here to allude to a recommendation which I hope the Association will not fail to leave in Liverpool, for the promotion of a scientific object of immense consequence to this port—the establishment of an Observatory in or near Liverpool. The adoption of such suggestions, while conferring an incalculable benefit on science, would rear a proud, unperishable, and bloodless monument to national greatness.

These statements might be a sufficient answer to a question, sometimes put in tones of captious sarcasm,—What has the Association directly contributed to the progress of useful knowledge? Without again appealing to the very admirable reports on the progress of science published in our Transactions; without again claiming merit for the suggestions and efforts already noticed,—I should fearlessly answer such cavillers, by an appeal to the value and number of the communications, which have occupied the different Sections, at each annual meeting, and which contain the application of pure science to important questions in Physics, or of experimental investigation to numerous branches of knowledge. I would point to the valuable researches which have been undertaken and completed at the request of the Association, among which it may be permitted to indicate the following memoirs:—The comparison of the standards of Linear Measure, made by the late Mr. Troughton, for the town of Aberdeen, and the Astronomical Society of London, which were confided to Mr. Baily—a comparison of much consequence, as the *standard yard*, by the same eminent artist, was lost in the fire which consumed both Houses of Parliament; On the Investigation of the Impact upon Beams, when struck by bodies of different weight, hardness, and elasticity, by Mr. Hodgkinson; On the Direction and Intensity of the Magnetic Force in England, Ireland, and Scotland, by Professor Lloyd, Major Sabine, and Captain James Ross; On the influence of Height above the Sea on Magnetic Intensity, by Professor Forbes—from which it appears that the horizontal intensity diminishes $\frac{1}{1000}$ of the whole, for every 3000 feet of vertical ascent; On the quantity of Rain falling at different heights above the surface of the Ground, made at York, by Professor Phillips, and Mr. Gray; On the determination of the mass of the planet Jupiter, by the Astronomer Royal; On the Horary Variations of the Barometer, Thermometer, Hygrometer, and Whewell's Anemometer, by Mr. Snow Harris—part of which has already appeared, and of which the sequel will be laid before this annual Meeting; On the Duty performed by Cornish Steam Engines, by Mr. Enys; On the Ratio of the Resistance of Fluids to the Velocity of Waves, by Mr. Russell and Mr. Robison—of which we expect to receive an account on this occasion.

We may also be permitted here to allude to some highly-interesting investigtions, still in progress, under the auspices of the Association, such as—Observations on the Temperature of Springs and Deep Mines, by Instruments procured and verified by the Meteorological Council, which are already placed in various districts of Great Britain and Ireland, and also in Peru, under the direction of our scientific associate, Mr. Pentland, and from which results most interesting to Geology are anticipated; On the Temperature of the strata at different depths near Edinburgh, by Professor Forbes, for ascertaining the rate of the transmission of Solar Heat downwards; A continuation of Mr. Vernon Harcourt's experiments on the effects of long-continued Heat on Rocks and other bodies; Experimental Investigations into the Fabrication of Glass, by the same gentleman and Dr. Faraday; A Systematic Catalogue of all the Organized Fossils of the British Islands, by Professor Phillips; An Experimental Determination of the Strength and other Mechanical Properties of Iron obtained by the Hot and Cold Blasts, undertaken by Messrs. Hodgkinson and Fairburn; Analysis of Iron in the different stages of its manufacture, and an Extension of the Tables of Chemical Constants, by Professor Johnston; Statistical Returns of the State of Education in our great towns; An Examination of the Statistical documents preserved in the India House, by Professor Jones; besides the discussion of numerous very interesting contested points in Natural History and in Medicine.

These are satisfactory evidences of the activity of the Association; but it has not scrupled also to afford pecuniary assistance, when such aid appeared requisite to ensure success. It is true, that the moderate sum, payable on admission into the Society, seems more suited to the finances of the majority of philosophers, than to the support of extensive enterprises; yet the numbers, annually desirous of admission, supply funds, adequate to important undertakings; and the power thus given to the General Committee is acknowledged to have been exercised with a sound discretion.

Without descending to minute particulars, it may be well to state some of the appropriations for various scientific inquiries.

The application to the French government already noticed, was accompanied by a vote of the General Committee of the Association to appropriate 500*l.* for a duplicate reduction of the Astronomical Observations, with a view to secure the utmost accuracy in these important computations. This offer proves the value attached by the Association to whatever can improve Astronomy, and the zeal which carries its scientific views even beyond the limits of the British Empire. This sum is still devoted to the reduction of Astronomical Observations. 70*l.* have

been devoted to the determination of a constant numerical expression for Lunar Notation, as deduced from the observations made with the Greenwich mural circle. 250*l.* have been appropriated for the Discussion of the Tides; besides 150*l.* voted last year for the Discussion of the Observations made on the Tides at Bristol. 100*l.* were set apart for meteorological instruments, and experiments on subterranean temperature,—the last a problem of the highest interest to Geology, as involving the question whether or not there be a general source of terrestrial heat, independent of solar influence. 500*l.* have been voted for ascertaining the permanence or fluctuation in the relative level of the land and of the ocean, on the coasts of the British Isles. This subject affords matter for the highest speculations in Geology; but it is doubly interesting to a maritime people, as affecting the permanence of our river navigation, and of our naval stations. 210*l.* were given to enable M. Agassiz to include the fossil fishes of our islands among his interesting Researches on Fossil Ichthyology; a publication which forms a new era in this department of Geology. 100*l.* have been assigned for Investigations on the Form of Waves, and the mode of their production. 150*l.* for the experiments on Vitrification, and the improvement of the manufacture of Glass. 80*l.* for experiments on Lenses of Rock Salt; a subject of much interest to Optics. 50*l.* for determining the specific gravity of Gases. 60*l.* for an experimental inquiry into the strength of Iron. 50*l.* for ascertaining the Duty of Steam Engines. 50*l.* for an inquiry into the Origin of Peat Mosses. 250*l.* for conducting various Physiological Researches. 150*l.* have likewise been voted for investigating the Statistics of Education in our large towns. While on this subject, I must not omit to state, that the active Statistical Societies of London and Manchester trace their origin to this Association; and that the laborious investigations of Colonel Sykes, on the Statistics of India, founded on materials chiefly collected by himself, and undertaken at the request of the Association, are now happily brought to a close, and will be presented to the Association.

These appropriations are exclusive of several minor sums devoted to the encouragement of investigations into various branches of Physics, Chemistry, and Natural History; making an aggregate of upwards of 2659*l.* already set apart from the funds of the Association, for scientific objects—a larger sum than has been appropriated, in so short a period, by any other Society, to purposes purely scientific.

While stating these facts, we ought not to conceal a circumstance, creditable to the disinterested zeal for the cause of science elicited by these grants. Though the votes have been liberal, this has never induced inconsiderate expenditure. In many instances, far less than the sums appropriated have been actually expended; and in various instances, the individuals intrusted with the funds have refused to draw on the Association, when their own labour could save its finances.

It has been usually considered a part of the duty of the Local Secretary, to give a short account of the Reports which are just published.

The first in the volume is the masterly report 'On Mineral and Thermal Waters,' by Dr. Daubeny. After glancing at the nature of atmospheric water, the author has pointed out the connexion of the foreign ingredients, detected in the atmosphere, with the production of meteoric stones, the formation of nitric acid under certain circumstances, and the presence of the organic principle found in air, even when collected on great elevations, to which the name of Pyrrhine has been given. He considers the existence of the elements of meteoric stones in the atmosphere as doubtful. The nitric acid may sometimes arise from the effects of electric explosion on its oxygen and nitrogen; at other times this union is seemingly produced by causes not yet ascertained. The researches of the celebrated Ehrenberg have shown, that pyrrhine probably owes its origin to the ova of polygastric *infusoria*, raised by evaporation and by atmospheric currents induced by changes of temperature. In considering the ocean, the author directs particular attention to its gaseous contents; as confirming or invalidating the opinion of Arago, that oxygen predominates in all waters, even to considerable depths. This law is well known to hold good in the more superficial portions of the ocean, and seems intended to support the respiration of aquatic animals; but the preponderance of oxygen at great depths, cannot yet be considered as absolutely determined, on account of the imperfection of the modes of obtaining unmixed water from such points. The water of springs is more especially the object of Dr. Daubeny's Report.

In considering the saline contents of mineral springs, he gives some ingenious speculations on the origin of these salts: especially of the carbonate of soda, of the sulphates, and of boracic acid. The common salt he derives from the same source as the saltness of the sea; and he considers rock-salt as a deposition from the waters of the ocean; a view confirmed by the presence in saline deposits, of iodine and bromine—elements first detected in marine productions. Dr. Daubeny ascribes the absence of these two bodies in the lowest and purest bed of the Cheshire rock salt, while they abound in the upper saliferous beds, as proofs that rock salt was deposited from a saturated solution. The salts of iodine and bromine, as well as the earthy muriates, from their greater solubility, would remain longer in solution; and thus be mingled with the more hasty mechanical deposits from the waters. The brine springs of Droitwich, which are found to contain neither iodine nor bromine, he also considers as derived from a salt deposited from a saturated solution.

The siliceous earth, so often detected in thermal springs, he conceives to be dissolved by alkaline matter, aided by a high temperature. Both alkali and silica may be afforded by felspathic rocks; and Dr. Daubeny conjectures, that silica may be more soluble in hot water at the moment of its separation from its combinations in the rock, or ere it has its aggregation increased, by assuming the crystalline texture. He states, that it may be interesting to try, whether hot water has a stronger action on such bodies as opal, in which the molecules do not seem to have a true crystalline arrangement, than on quartz. Since I came this time to Liverpool, I subjected a fragment of wood-opal for fourteen days to a temperature estimated about 280° Fah., in the boiler of a fixed steam-engine; but it had neither lost nor gained the smallest weight in that time.

The author combats the opinion of Anglada on the origin of the organic matter termed *Glairine*, now found to be a very common ingredient of thermal springs. This substance Anglada supposes, with little probability, to be derived from the interior of the earth; while the observations of our author on this substance, as collected from above fifty springs, especially from the thermal sources of the Pyrenees, show, that *Glairine* is probably derived from the decomposition of organic bodies, such as *conferva* and infusory animalcules.

The author's speculations on the source of the heat of thermal springs, partake of his views on the origin of volcanoes; namely, that it depends on the penetration of water, through fissures in the external crust of the globe, to the regions where he conceives the elements of earthy and alkaline bodies to exist: that the intense heat, generated during the oxidation of these elements, converts a portion of the water into steam; which, under compression, obtains a high temperature, acts on various earthy bodies, and communicates its heat to subterranean waters which issue in thermal springs. This view he supports by numerous instances observed by geologists; especially by Professor Forbes in the Pyrenees, where thermal waters gush out in the vicinity of disruptions, or upheavings of strata by ignigenous rocks. The author believes that, unless in countries agitated by volcanic action, the temperature of thermal springs is subject to little variation; and, that where the contrary has been alleged, it may generally be ascribed to the imperfection of the thermometers employed.

The temperature of copious springs has generally been observed to vary little, and is about the mean temperature of the country where they occur. Thus the magnificent fountain at Vaucluse has the mean temperature of that part of France, and scarcely ever varies one degree of Reaumur. It is however worthy of remark, that I found the temperature of St. Winifred's Well, the largest spring in Britain, by different observations during twenty years, to experience variations of more than four degrees of Fah., always to have a temperature several degrees above the mean of Flintshire, and at all seasons superior to that of another very large spring, Fynnon assa, about five miles distant. The variations may perhaps arise from surface water, directly finding its way into the Holywell spring; but its constant superior temperature may be accounted for, on Dr. Daubeny's principle, from the disturbances in the strata produced by the numerous mineral veins in the adjacent Halkim Mountains.

The second report is 'On the Direction and Intensity of Terrestrial Magnetism in Scotland,' by Major Sabine.

The experiments were made at numerous stations, both by the statical method of Professor Lloyd, in which the dip and intensity are ascertained by the same instrument, and by Hansteen's method, of measuring intensity by the number of horizontal vibrations in a given time. It is interesting to know, that the intensities estimated by both methods nearly correspond; and that we therefore may place confidence in either mode of observing, when allowance is made for changes in the force of magnetism in the needles employed. Major Sabine experienced, on several occasions, what has been remarked by other observers, that magnetical experiments are liable to be affected by the vicinity of Trap rocks. This was particularly noticed by him at Oban and Loch Scavig, so as to render his observations at the latter of no utility for his calculations. Two of the most familiar examples of this quality of ignigenous rocks are afforded by the powerful effect of a column of the Giant's Causeway, as mentioned by Professor Lloyd; and by the strong polarity of the basaltic cap of Arthur's Seat, near Edinburgh, which is capable, in more positions than one, of causing complete inversion of poles of the pocket compass. These instances show how carefully the vicinity of considerable masses of Trap rocks should hereafter be avoided, in all delicate experiments on magnetic dip and intensity: for the errors they occasion may be more considerable than the effect of a ship's local attraction on azimuths, and are far less easily compensated.

Major Sabine has considered it best to give no other designation, on his chart, to the Isodynamic lines in Scotland, than what expresses their relation to each other, until we have more fully investigated their relation to magnetic intensity in England. The differences between the deductions, in regard to the Isodynamic lines in Scotland and in Ireland, are very considerable, and apparently too great to be due to any difference in the lines themselves: but observations are still too few to afford accurate results.

In a former volume of our Transactions, appeared a valuable report on North American Geology: in that just announced is an excellent essay on the Zoology of that portion of the globe, by Dr. Richardson, the intrepid friend and companion of Sir John Franklin, in their hazardous exploratory expeditions to the shores of the Arctic Ocean. After some general remarks on the climate of North America, he presents us with an extensive Table of Mean Temperatures, calculated for periods of six and three months throughout the year, for the hottest and the coldest months, and for the months with a mean temperature above 52° Fah., taken at forty-four different stations, and collected from his own and Franklin's observations, combined with those of Humboldt, Ross, Parry, and Scoresby. The results are very important; and show, in a striking manner, the very erroneous deductions on the mean temperature of any place, if investigated by Mayer's formulæ, especially in very low or very high latitudes.

The geographical position of Mexico constitutes that the point in which the Faunæ of the northern and southern regions meet; and, hence, it is the place in which the general laws, regulating the distribution of animals, can be most satisfactorily studied. There the wolf of a northern climate is seen with the monkey of tropical regions; the bunting and the titmouse nestle near the parrot and the trogon; the phalarope of the North seeks its food on the same beach as the jacana and the boatbill of Brazil.

Dr. Richardson states, that, though colonization has, in America, restricted the range and modified the migrations of wild animals, we have no evidence that a single species has been there lost within the records of history. The quadrumana, or monkeys, of America are also peculiar to that continent. None of them have what may be called a perfect hand, with the thumb opposed to the fingers. Their thumbs are

small, sometimes only rudimentary, or even wholly wanting. Not a single ape—not one true baboon is to be found among them; but many are furnished with prehensile tails, admirably adapted for animals moving among thick forests, and almost as serviceable for grasping as the proboscis of the elephant.

Almost all the Mammifera, considered as common to the New and Old Worlds, belong to the order of Carnivora. Yet, it is by no means improbable, that a minute examination of species, now considered as the same, may detect specific differences among them. I would particularly recommend attention to the skulls of animals. My late ingenious young friend, Robert Jameson, of Edinburgh, had acquired great tact in discriminating the Carnivora, in particular by the form and position of the sutures uniting the bones of the face, which differ much in each species. It is believed by many naturalists, that the proportions of the skulls of Indian birds, in other respects similar to our own, as compared to their bodies, differ from those of Europe. Similar differences may occur in other parts of the skeletons of quadrupeds, which have escaped the superficial examiner, yet sufficient to constitute specific characters. This would be particularly valuable in determining the species of weasels and amphibious Carnivora, which, at present, are very perplexing to the naturalist.

All the existing *marsupial* animals are confined to America, Australia, and some other South Sea islands: yet, at one period, animals of this order must have been very generally distributed over the earth, as their bones occur everywhere in a fossil state, and are formed in the oldest deposits of mammiferous remains.

The number of Rodentia in North America is great, and all seem to be peculiar to the New World: of the Edentata, one only is found in North America. Two or three species occur in Africa and India; all the rest are South American. It is singular that of the existing Pachydermata, two species only are considered as indigenous to America—the tapir and the peccary; and of these, the last only is found in North America. Yet no region can boast of more numerous, or more gigantic species of fossil animals of this order—as elephants and mastodons—and, what is remarkable, though the present race of horses is acknowledged to be not indigenous, fossil bones of the horse were found on the N.W. coast by Capt. Beechy mingled with those of elephants. Of the Ruminantia, two only seem to be common to the Old and New World—the reindeer and the elk—unless we admit that the argali of Siberia is the same as the sheep of the Rocky Mountains.

The Cetacea, as might be expected from their mode of life, may be considered as common to both worlds. The *Rytina Borealis* and *Manatus Americanus* are formed in North America, but not in the seas of Europe. Temminck estimates that we have 930 well ascertained, and 140 doubtful species of Mammifera; of these 207 are in the New World, and 169 in North America. The birds of North America are most numerous, and have been illustrated by the successive labours of Pennant, Wilson, the Prince of Musignano; but, above all, in the Fauna Boreali-Americana of Richardson and Swainson, and the superb work of Audubon. The similarity between the birds of Europe and North America is marked by one-third of the species being common to both Faunæ. These are chiefly to be found among the Grallatores and Natatores, two-thirds of which orders are common to both: of the order Rapaces several are common to both continents. The Insessores are very numerous, and a great number are peculiar to America. The Rosores, in all countries, are little disposed to migrate; and almost all of this order, found in America, are peculiar to it, with the exception of some pigeons and a few Arctic grouse.

The Reptilia of North America are exceedingly numerous. All, with the exception of some sea-turtle, are distinct from those of the Old World. Two genera equally fitted to live in water and in air, as possessing both gills and lungs, and represented by the *Siren lacertina* and *Meopoma gigantea*, which abound in North America, have only one analogous animal in the Old World, the *Proteus anguineus* of the lakes and caves of Carniola.

Many species of the fishes of the American seas are found elsewhere; but the only fresh-water fish, common to both worlds, appears to be the pike. Yet, it is singular, that it does not occur in the waters to the west of the Rocky Mountains, although there the two continents are more approximated. Some of the family of the Salmonidæ and Clupiadæ, which visit America, have much resemblance to those of Europe.

This Report is an excellent specimen of the method of comparing the Faunæ of distant regions, and presents a model of a philosophical disquisition on the geographical distribution of animals.

The Association has, at different times, received three able Reports from Professor Challis, of Cambridge, on the Mathematical Theory of Fluids. In the first he showed how the application of mathematical analysis to investigating the properties of an imaginary fluid, supposed incompressible, or so compressible, that the density should always be proportional to the pressure it sustains, admits of comparison with facts observed in the equilibrium and motion of water, or in the existing mechanical qualities of air. In the second, the author considered the modifications which these theories had, in later times, sustained by the introduction of certain molecular hypotheses on the constitution of matter, and how a comparison of the consequences of these hypothetical speculations, with experimental results, served to establish the basis of the mathematical reasoning, and to make known properties and conditions of bodies not cognizable by our senses.

The present Report treats of several very important points in the Mechanical Theory of the Atmosphere. He considers Mr. Atkinson's* attempt to ascertain the law of variation of temperature, at different heights in the atmosphere, as requiring, for its establishment, a more extensive series of observations, over a greater portion of the earth's surface than we now possess.

The difference between the velocity of sound, as determined by experiment, and Newton's deduction from Boyle's and Mariotte's law of elastic fluids, amounting to one-sixth of the whole, has given rise to many attempts to solve the problem, especially by Euler, Lagrange, and Laplace. The latter gave the true solution of the discrepancy—namely, that it arises from the evolution of heat, and its absorption, which accompany every sudden compression or expansion of air. The application of analysis, to afford a formula of correction, was first attempted by Biot and Laplace, and more lately by Ivory; but when we compare the theoretic deduction with the best experiments on the propagation of sound, by Moll and Van Beck, at Utrecht, by Goldingham at Madras, and Parry and Foster in the Arctic regions, the slight discrepancies between experiment and calculation are more to be attributed to some imperfection in our formula than to error in experiments, which in their results agree so nearly, though made under very different circumstances.

Under the head of Theories of Elastic Fluids, the author has introduced some valuable remarks upon the memoirs of Poisson, on the equilibrium and motion of elastic bodies, on the equilibrium of fluids, and the pressure of fluids in motion; and also on Laplace's theory of Capillary Attraction; for which I must refer to the Report.

We have next two reports on the Comparative Botany of Scotland and Ireland, by Mr. Mackay and Professor Graham, of Edinburgh. The first indicates the more remarkable plants that characterize the neighbourhood of Dublin and Edinburgh. In the second, Mr. Mackay points out the effect of climate on the Flora of Ireland. Ireland, it is true, has fewer species of plants than Great Britain, and possesses fewer alpine plants than Scotland. The position and moister climate, however, put me in possession of many plants not found in Great Britain, but of species occurring in Spain and Portugal, among which may be noticed *Erica Mediterranea, Erica Mackiana, Pinguicola grandiflora, Arbutus unedo, Menziesia polyfolia.*

The Reports from the London and Dublin sub-committees, on the Motions and Sounds of the Heart, in this and the last volume, will interest the physiologist and the physician. Ever since the application of the stethoscope, by Laennec, to the investigation of pectoral diseases, the sounds of the heart have been anxiously explored—its normal sounds studied, and its abnormal *bruits* eagerly inquired into, as important diagnostics of health and disease. The causes of those have been matter of dispute; the investigation was recommended by the Association; and a sum appropriated for the expense of experiments on the subject. The Reports are the results of the labours of two sub-committees, who agree on the principal points, viz., that the first sound is produced during the systole, or contraction of the ventricles; and that the second sound is produced by the sudden check, which the action of the semilunar valves gives to the current of blood impelled against them, by the elasticity of the arteries. In the second Dublin Reports, the abnormal sounds are illustrated by some ingeniously-devised experiment: but both sub-committees admit, that the motions and sounds of the heart require further investigation.

The Dublin Committee, on the Pathology of the Brain and Nerves, express their opinion, that to arrive at any accurate conclusions on so extensive and difficult a subject, a very large number of cases must be first submitted to examination, their symptoms during life accurately noted, and minute examinations instituted afterdeath. One hundred and seventy-eight males and two hundred and ninety-four females, labouring under nervous affections, are in the Dublin House of Industry and Hospitals—of whom forty-one have already been accurately examined, for the object just alluded to.

The results of the Discussion of the Observations on the Tides, obtained by means of the grants of the Association, have been reported by Mr. Lubbock.

Mr. Dessiou was employed to discuss the Tides observed at Liverpool, so as to ascertain the diurnal inequalities in their height, and also to classify the errors of prediction for a year in Liverpool and at the London Docks. The result is, that Daussy's deduction from the observations at Brest is confirmed, viz. that the height of high water is diminished when the barometer is high, and increased when it is low.

The various discussions of nineteen years of observations at the London Docks, amounting to 13,370, for the purpose of deducing the diurnal irregularities, and examining the effects of the moon's transit immediately preceding high water, and those of the two previous days, lead to the conclusion, that Bernouilli's theory of Equilibrium "satisfies the phenomena, nearly, if not quite, within the limits of errors of the observations," and that it leaves very little to be otherwise accounted for.

A short statement is made by Professor Powell, of Oxford, on the Determination of Refractive Indices for the definite rays in the Solar Spectrum, from direct observation. The investigations recommended in the third Report of the Association, have been commenced by Professor Powell, who continues his observations.

Mr. Hodgkinson reported from the London Physiological Committee, that their investigations have not established the views of Lippi, respecting the communications of the absorbents with the veins; but they do not warrant a rejection of his observations, nor amount to any proof that the thoracic duct is the sole medium of communication between the lacteals and the veins. Direct communications between absorbents and veins have been observed by the reporter: but he is disposed to consider these as deviations from the normal structure.

A short Report on the best methods of ascertaining Subterranean Temperatures, and the proper form for Registers of such observations, is published by a Committee appointed for the purpose.

The last Report in the volume is the very profound Examination, by Sir William Hamilton, of the Validity of Mr. Jerrard's proposed method of *Transforming and Resolving the higher degrees of Equations*, as contained in his 'Mathematical Researches.' Mr. Jerrard's method may be characterized as consisting in rendering the problem indeterminate, and in employing this very property to decompose certain of the conditions into others, for the purpose of avoiding that elevation of degree, that would otherwise be the consequence of the elimination. The ingenuity of the principle, and the talent displayed in the researches, are freely admitted by Sir William, who contends that the process is valid, as a general and unexpected transformation of equations of elevated degrees, though it fails as a method of resolving them; and who thus sums up the result of his investigations on the subject.

* Trans. of Royal Astron. Soc. v. 2.

"This method of *decomposition* has, however, conducted, in the hands of Mr. Jerrard, to *transformations* of equations, which must be considered as discoveries in algebra; and to the solution of an extensive class of problems in the analysis of *indeterminates*, which had not before been resolved: the *notation*, also, of *symmetric functions*, which has been employed by that mathematician, in his published researches on these subjects, is one of great beauty and power."

On the very valuable matter contained in the proceedings of the Sections, time will not permit me to enter, and I must refer you to the volume just published.

In conclusion, allow me, in the name of my respected colleagues and of our Liverpool associates, to offer a sincere and hearty welcome to the distinguished strangers, whose presence confers additional interest to this meeting: and secondly, to congratulate the town of Liverpool on the exertions it has made, worthily to receive an Association, which, aiming at the diffusion of a general taste for scientific investigations, and their application to the improvement of society, seems calculated to perform an important part in the future destinies of our country—which, as co-operating with all other scientific bodies, and the rival of none, but including in its lists representatives from each—which, distinguished by the freedom of its discussions, the liberality of its assistance, and the importance of its recommendations, has been happily characterized, by an eloquent secretary of a former year, as a *Fourth Estate in the Realm*, and aptly designated HER MAJESTY'S PARLIAMENT OF SCIENCE.

When the Secretary had concluded, Mr. John Taylor read the financial report, from which it appeared, that a sum little short of 1000*l.* had been expended under the sanction of the Committee of Recommendations in furtherance of scientific inquiries—that the total amount of the property and funds of the Association had notwithstanding increased to 5,284*l.* 14*s.* 6*d.*—and he added, that the number of tickets issued at the close of the Bristol meeting, was 1350, and the number already issued in Liverpool, 1420.

Mr. Murchison rose to remind members of the new arrangements made by the General Committee, for the meetings of that body on Thursday and Saturday; after which, the general meeting of the Association was adjourned to Wednesday evening.

TUESDAY, SEPT. 12.

SECTION B.—CHEMISTRY AND MINERALOGY.

Dr. Faraday in the chair.—The President addressed some observations to the Section, expressive of his feelings on taking the chair. He stated, that he considered other individuals present would fill the chair of the Section with greater ability than himself, seeing that his pursuits had, for some time back, led him away from Chemistry into purely Physical researches. He suggested, however, that some advantages resulted from this circumstance, inasmuch as being neither devoted exclusively to Chemistry nor Natural Philosophy, he should be the better able to hold the balance evenly between the cultivators of both.

Mr. Griffin then exhibited a number of articles of chemical apparatus, adapted for experiments on the small scale. Some of them were new and ingenious; but we cannot help thinking that the object of the author, though no doubt a very legitimate one, was in some degree foreign from the diffusion of science. Dr. Faraday bore testimony to the value of the apparatus, by which, at the most economical rate, the science of Chemistry could be prosecuted and diffused to an extent which, without its assistance, would be impracticable.

Mr. G. Bird, having complained that his paper of Monday was read contrary to an arrangement made in the Committee, was permitted by Dr. Faraday to repeat, *vivâ voce*, the nature of his principal experiment, and the result which he conceived to be established by it. Having already given a full report of the paper, we do not think it necessary to return to it. We may, however, observe, that Dr. Faraday suggested that it would be important as a preliminary, for Mr. Bird to ascertain whether, independent of the galvanic couple, the sulphate of lime did not contain something capable of originating the deposition of the metallic copper; and he seemed to entertain a doubt as to whether, in point of fact, in such experiments, there is ever any deposition of metal, except at the negative electrode.—Some observations were then made by Mr. Henwood and Mr. Stephens, which concluded the discussion.

Mr. Hartley read a paper, 'On the Corroding of Iron by Salt Water.' The object of the paper was to show that brass protects both bar and cast-iron in a very perfect manner. The brass did not appear to have undergone any action, which, as stated by the President, is rather opposed to received notions of electro-chemical action.

Dr. Andrews next read a paper, 'On some Singular Modifications of the Ordinary Action of Nitric Acid on certain Metals.' Bismuth in nitric acid of specific gravity 1.4, was rapidly acted upon, but this action immediately ceased when the bar was touched by platinum. On removing the platinum from the liquor, the bismuth will sometimes begin again to dissolve; at other times, its surface will become covered with a black crust, which is soon removed by the acid; but the metal, though now exhibiting a beautifully-polished surface, is no longer acted upon by the acid, or, at least, is dissolved only with extreme slowness. Thus, a slip of metal, which, in its ordinary state will require only a few seconds to complete its solution, will, when thus slightly modified, resist, for many hours, the action of the same acid.

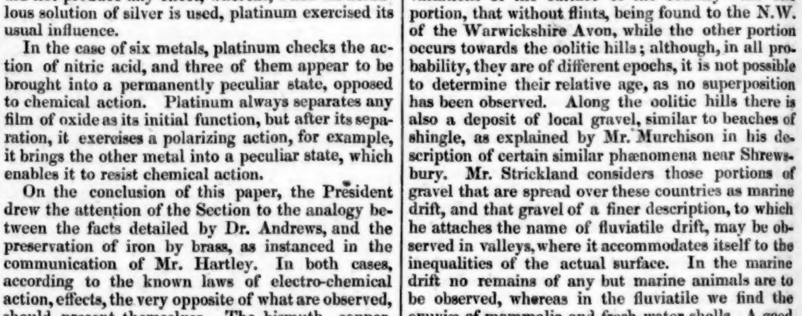
Copper and tin present similar phænomena, but zinc, when treated in the same way, has its oxidation and solution not arrested, but merely retarded. Arsenic was found to present a singular anomaly when heated in nitric acid, so as to give rise to effervescence: the contact of the platinum in the usual way did not produce any effect, whereas, when an acidulous solution of silver is used, platinum exercised its usual influence.

In the case of six metals, platinum checks the action of nitric acid, and three of them appear to be brought into a permanently peculiar state, opposed to chemical action. Platinum always separates any film of oxide as its initial function, but after its separation, it exercises a polarizing action, for example, it brings the other metal into a peculiar state, which enables it to resist chemical action.

On the conclusion of this paper, the President drew the attention of the Section to the analogy between the facts detailed by Dr. Andrews, and the preservation of iron by brass, as instanced in the communication of Mr. Hartley. In both cases, according to the known laws of electro-chemical action, effects, the very opposite of what are observed, should present themselves. The bismuth, copper, &c. should oxidize quickest when in contact with the platinum; and if, as would seem demonstrated by Mr. Hartley, brass protects wrought and cast-iron, the brass itself should be acted upon with increased rapidity. The solution of these anomalies, he conceived quite within the range of science in its present state, and he urged upon the members of the Section the necessity of studying the phænomena in question, as their explication would constitute a very valuable addition to the existing state of our electrical knowledge.

SECTION C.—GEOLOGY AND GEOGRAPHY.

Professor Sedgwick, President, in the chair.—Mr. Strickland read a memoir on formations of gravel in the counties of Warwick and Worcester. He commenced by pointing out the great variety of gravel which is found in England, and its varied positions, it being found sometimes in the vicinity of the rocks from which it is derived—at others very far removed; in some places regularly stratified—in others forming outliers on the summits of hills. In some parts we find marine organic remains; in others bones of Mammalia and lacustrine shells. All these gravel formations are, however, unconformable to the beds on which they rest, and it is difficult to determine their age. The researches of Mr. Murchison were mentioned with great praise, in elucidating many points respecting the gravel of Wales and the adjoining counties; one point established being the existence, in former ages, of a marine current, from Cheshire to Gloucestershire, which had brought with it gravel in its course. The gravel formations described by Mr. Strickland, were divisible into two great portions, one being with flints, the other not containing any; and they occurred independent of the minor variations of the surface of the country—the one portion, that without flints, being found to the N.W. of the Warwickshire Avon, while the other portion occurs towards the oolitic hills; although, in all probability, they are of different epochs, it is not possible to determine their relative age, as no superposition has been observed. Along the oolitic hills there is also a deposit of local gravel, similar to beaches of shingle, as explained by Mr. Murchison in his description of certain similar phænomena near Shrewsbury. Mr. Strickland considers those portions of gravel that are spread over these countries as marine drift, and that gravel of a finer description, to which he attaches the name of fluviatile drift, may be observed in valleys, where it accommodates itself to the inequalities of the actual surface. In the marine drift no remains of any but marine animals are to be observed, whereas in the fluviatile we find the exuviæ of mammalia and fresh-water shells. A good example occurs in the valley of the Avon, from



Rugby to Tewkesbury, of which the above is a section; A being a hill of some elevation, and capped with marine gravel; the fluviatile occurring at B, and the usual river alluvial matter at C, while the lias is the formation in which this occurs. Mr. Strickland suggested, that further observations should be made upon the great marine drift, whether any point of junction can be determined of its two portions; also the existence in it of shells and bones, and the search, particularly for bones of Mammalia; and where fresh-water shells occur,—its distance from rivers, and its greatest height above them.

Mr. Sedgwick observed, that it is difficult to generalize respecting these formations, as they presented so many variations. He had seen gravel on mountains 2,000 feet high. Erratic blocks, he considered, could not be of fluviatile, but of marine origin, and organic remains of large animals were not likely to be abundant in gravel carried by currents of the sea, from the destruction caused by their violent action. Animal remains had been found in the clay gravel of the east of England; but this gravel he conceived as differing from that in other parts of the country.—Sir Philip Egerton said, that Mr. Strickland's flintless gravel, occurring to the N.W. of the Avon, could be only a partial formation, as he had observed, that in Cheshire the gravel always contained flint. At Cocknell he had obtained two grinders of an elephant, and many *marine* shells, and many like shells in other places, all of existing species, and occurring often with pieces of rolled coal.—Mr. Phillips stated, that one of the questions proposed by the Association, was the determination of the phænomena of the English gravel formations, but to the present time suf-

ficient evidence had not been collected. He alluded to Mr. Murchison's opinions, that a strait formerly divided England from Wales: into this strait gravel might be drifted from both sides; and Mr. Murchison had discovered, in Wales, local covered by erratic gravel. He himself had discovered in a valley of the Yorkshire Wolds at an elevation of 600 feet, gravel drifted from Cumberland, and containing bones of the elephant. He mentioned, also, the fresh-water gravel formation in the vale of York, in which Mr. Harcourt had caused sinkings to be made, and had found, at the depth of 22 feet, the bones of Mammalia along with fresh-water shells.—Mr. De la Beche spoke, also, of the difficulty of the question. On the Blackdown Hills he had observed a peculiar drift; on adjoining hills a drift of a different kind, and in the intervening valleys a mixture of the two, containing the bones of Mammalia.—Mr. Clark also mentioned the difference of the gravel upon the plastic clay and the chalk of Dorsetshire. In that county he had observed two beds of gravel, one overlying the other.—Mr. Trimmer mentioned the occurrence of marine shells at an elevation of 1,400 feet in Caernarvonshire; also of gravel at a lower elevation on limestone bored by shells.—Mr. Murchison stated his determination of the local gravel in the part of the country which he has termed Siluria; in this drift no shells are to be found. The drift from the north, he conceives must have been submarine, and that, in all places, it may have unequal distribution, from the unequal elevation of the subjacent land. He alluded to his communication at the Bristol meeting respecting the elevation of Siluria at a different epoch from the other parts of Britain, as proved by the difference of the gravel formations.—The discussion respecting gravel was closed by Mr. Sedgwick. He had traced the gravel of the central parts of England to its northern sources; and he instanced a singular phænomenon of a mountain, near Buttermere, which appeared to be waterworn by a stream passing over it. The recent elevation of Siluria, he conceived, was proved by the morasses and lakes in its lines of valleys, which valleys, although shaped under the ocean, had been evidently modified by existing waters. He concluded by asserting, that all examination of nature, by means of accurately ascertained facts, must agree with moral or scriptural interpretation; and that we need have no fear as to the one clashing with the other, as truth cannot oppose truth, but must, in all cases, be coincident.

The next paper, brought before the Section was by Mr. Mallet, 'On the Mechanism of the Motion of Glaciers.' This gentleman pointed out, that many phænomena of these singular masses had been hitherto overlooked; and, although described by many eminent observers, no solution had been given to the question of their movement, but that of their weight, which he showed could have only a partial operation, as they often rest on rugged beds, and these not always of much inclination. He proposed a very ingenious explanation of their movement by means of hydrostatic pressure, arising from the fact of the lower part of the glacier being of a higher temperature than the upper: this causes a melting of the under part, and a consequent raising of the mass in a perpendicular direction to the earth's surface, while its descent was at right angles to the inclined surface—a progressive motion downwards ensued, following the law of the resolution of forces. Mr. Mallet then spoke of certain causes of the rents and fissures in glaciers, these being often convex downwards, owing to the operation taking place in the middle part of the mass, which descends soonest, while the whole is held in its place by the upper and lower extremities; also tubular fissures are formed by blocks of stone, sinking by degrees in the glacier, owing to their higher temperature melting by degrees the surrounding ice. He then alluded to the singular accumulations of detritus on the glaciers, which are locally termed *moraine*, and are formed by *éboulements* in winter, and covered by the snow. These, Mr. Mallet, found to assume linear directions, parallel to the axes of the glacier; and, from the regularity of these arrangements, he conceived it possible to discover the site of old glaciers from the moraine, which had remained on the ground after their destruction.

The Marquis Spineto made a communication respecting the Geology of the Desert between Suez and Cairo. In this desert travellers have always suffered great inconvenience from the want of water, and this was likely to prove a serious obstacle to the proposed communication by this route to India. In order to overcome the inconvenience, Mr. Briggs, the British Consul, employed a Swiss engineer to bore for water. Mr. Gensberg, the engineer, caused the first boring to be made in the Valley of Kejche, but being unsuccessful, he transferred his operations to the Valley of Candelli, where water was found in clay underlying a calcareous rock. Considerable ingenuity was shown in the excavation. Besides the usual boring downwards, lateral openings were made to increase the supply of water: borings were made in other situations, and very singular results obtained. A great variety of strata were penetrated, and this variety existed even within a limited distance of superficial extent: thus, in one place marine sand was found; and a little way off, terrestrial or desert sand: gravel occurred only in one spot. But the most singular geological phænomenon was the existence of granite over clay, in which good water was obtained. The Marquis mentioned that a notice of the intention of the British Consul to bore for water, appeared in the first volume of the Journal of the Geographical Society, but that the communication now laid before the Section, was the first notification of the results obtained.

Mr. Horner exhibited to the Section a drawing representing some of the Geological phænomena in the neighbourhood of Christiania in Norway, and read a letter from Mr. Lyell in explanation. This part of Europe received, some years ago, an examination from M. von Buch, and the results were at that time presented by him to the scientific world in such a manner, as to excite much attention, and to stagger many in their attachment to the Wernerian Theory, then very prevalent. Von Buch had discovered granite overlying fossiliferous strata outstepping the bounds prescribed to it by Werner, so that his disciples were obliged to invent a creation of it at a later date, to explain the Norwegian phænomena. Mr. Lyell has lately examined this interesting locality, and has found the junction of the granite with the fossiliferous rocks well determined; also that it not only sends veins into them, but that it actually in some degree overlaps them, the strata rather turning up from it. The granite sometimes became syenitic, passing into trap porphyry; and it has altered the appearance of the adjacent rocks, making them truly *metamorphic*. These fossiliferous rocks rest upon gneiss, and may be referred to the Silurian system of Murchison. The granite in the same vicinity comes in contact with the gneiss, which rock has frequently indications of having been disturbed before the deposition on it of the Silurian rocks, and it bears on its surface marks of scoring and abrasion. The posterior formation of the granite is proved by its veins in the adjacent rocks, and by the fragments of gneiss in the Silurian strata. The entire phænomena are of the highest interest,—they must have been originally submarine, as the disturbances would have been vastly greater, had they not taken place under a pressure of perhaps some miles of ocean in depth, and perhaps also with other strata upon them: the gneiss, under such a pressure, may have been only softened. Near Christiania, many dykes, evidently of volcanic origin, also occur; these are of syenite passing into greenstone, and are evidently fissures filled with injected matter.

Mr. De la Beche observed, that it was now generally acknowledged that granite is of all ages, and that if it do not occur in dykes, its dome-shaped masses must manifestly have occasioned dislocations in the masses through which they were protruded.—Mr. Greenough was still unwilling to admit the igneous origin of granite as distinct from gneiss and mica slate; he conceived they must all have had the same origin, whether aqueous or igneous.—Mr. Clarke mentioned his having witnessed, in the department of Calvados, a direct passage of granite into gneiss, and thence into mica slate and clay slate.

The last communication was by Dr. Traill, 'On some points of the Geology of Spain.' At the Dublin meeting of the Association he had made some remarks on the same country. On the borders of Arragon he had observed oolitic rocks; new red sandstone also occurred there, covered in some spots by a conglomerate; near Saragossa gypsum is found with limestone; and in the same vicinity are tertiary formations. On approaching the Pyrenees from Catalonia, rounded pebbles occur in abundance, and at Barcelona are alluvial beds. At Montserrat he found clay slate, with conglomerate resting on it, succeeded by sandstone, dipping towards the valley containing the salt mines of Cordova described by Dr. T. in the Geological Transactions. The Doctor exhibited a drawing of these mines, where the part already worked affords an escarpment of 400 feet of the purest salt, besides an under portion not yet touched. Near the Pyrenees he found veins of trap. On the Spanish side of these mountains, clay slate and limestone occur; on the French side clay slate only, while granite is found in the central portions.

The President called the attention of the Section to a collection of fossils from the mountain limestone, lying on the table, and which Mr. Gilbertson had left for inspection.

Section D.—ZOOLOGY AND BOTANY.

Mr. Macleay in the chair.—The President observed that he was about to read a letter upon a subject that had excited some interest in the scientific world, on account of the difference between two eminent naturalists,—Mr. Thompson and Professor Rathke of Berlin. He remarked, that at first sight it appeared strange, that animals apparently so high in the scale of organization as the crabs and lobsters should undergo the same changes in the course of their developement, as the class Insecta. Mr. Thompson had observed, that the young of the barnacle shell were deposited by the parent in a free state, and that they afterwards became fixed; and the same fact had been pointed out to him by that zealous naturalist Mr. Darwin. It was therefore not extraordinary Mr. Thompson should take the view he did of the developement of Crustacea. The letter he had on this subject was from Capt. Ducane, of Southampton, who, from his contiguity to the sea, had been led to investigate this subject. He had obtained the ova of what he supposed to be the common prawn: after keeping them in water a little time, they produced a number of minute diaphanous animals, altogether different from the full-grown prawn. These ova were obtained from a ditch open to the influx of the sea, but the water was only slightly brackish. Captain Ducane therefore proposed to call this animal the "Ditch Prawn." The letter was accompanied by a drawing, exhibiting the various changes which the Captain had observed to take place in these animals during the first three days after their leaving the ova, none of his specimens living a longer period.

The President observed, that the subject of the developement of Crustacea was a most interesting subject. He questioned whether many of the received species of that class were anything more than one animal in its several stages of developement.

Dr. Richardson inquired if it might not be possible for Capt. Ducane to have mistaken some parasitic animals for the young of the prawn.—The Rev. F. W. Hope was inclined to think the animal alluded to belonged to the shrimp, and not to the prawn family. On the Norfolk and Suffolk coast, the ditch shrimp was common. He believed crustaceous animals themselves were frequently parasitic.—The President replied, that the ova obtained by Capt. Ducane were too abundant to be the produce of a parasitic animal, and the results too constant to lead to such a supposition.

Mr. Halliday laid upon the table some plates of the Argas Persicus, exhibited yesterday by Dr. Traill; he stated that there were two genera, the Argas and Ixodes, that produced these poisonous bites.—The President observed, that 'bite' was an improper term for the wounds of these animals. They were produced by the introduction of their long serrated proboscis, and the ill effects frequently attendant on these wounds, he thought, arose from the violent extraction of this serrated rostrum.

Mr. Babington, one of the Secretaries, then read a 'Notice, with the results of a botanical tour in Guernsey and Jersey.' Very little, he stated, appeared to be known of the botany of the Channel Islands; the only notices he had seen were those of Woods, Christy and Trevelyan, which were very scanty. He had spent two months on the islands, and collected about 500 specimens, and from a list from Mr. Saunders, nursery-man, of about 225

others. Of plants not before recorded, he had found the following: *Hypericum lancifolium, Neottia æstivalis, Sinapis incana, Mercurialis ambigua, Arthrolobium ebracteatum, Atriplex rosea.* He also gave lists of plants rare in England, and common in the Channel Islands, and of those common in the former, and not found in the latter.—Professor Lindley observed, that he had seen a list of the plants of these islands, drawn up by Lagasca, a Spanish botanist, but it was not so comprehensive as the one at present laid before the Section.

Mr. Allis then read a paper 'On the sclerotic Bones of the Eyes of different Birds and Reptiles.' He stated, that he believed the subject of his paper had not received much attention from comparative anatomists. With regard to their number, Cuvier had stated them to be twenty, but he had never found more than seventeen, and sometimes even only one. He then quoted the observations that had been made on this subject by Blumenbach, Cuvier, Carus, Yarrell and Buckland, and proceeded to state, that "the shape of the individual bones is so various, that it cannot be given in any general terms; the external edge of the bones is, in most instances, beautifully serrated, but the serration is not visible in the bony ring: this serration being generally destroyed by the process of boiling that is necessary to their preservation. The rings generally overlap each other, there being a depression on the under side of one bone, and a precisely corresponding one on the upper side of its fellow; so that when overlapping each other they present nearly an even surface, having one bone with both depressions on its inner surface, and forming an interior key to the arch, another, having two depressions externally, and forming an exterior key. They form a defence and protection to the eye, and those birds which are pugnacious, or have a peculiarly rapid flight, or vary their attitude in flying, &c., have the sclerotic rings of larger size and more convex form, and are of greater strength; the same remark holds good with respect to water-birds. Another use of these bones, is, altering the convexity of the cornea, as mentioned by Dr. Buckland." He then exhibited a great number of specimens of these bones, and observed, that in the Eagles and Vultures they were strong and large, and varied in number from fourteen to sixteen; in Owls, soft and porous, and not hard, as Cuvier had stated; in the Gallinidæ the number varied from thirteen to seventeen; in the Columbidæ they were small and feeble; in the Ostrich tribe they were large; in the Grallæ small and feeble; in the Scansores the same, and twelve or thirteen in number; in the Swimmers they were weak and small, and from twelve to sixteen in number; in Divers strong and large, and twelve to fifteen in number; in the Passerinæ they varied considerably, but were generally weak; in Reptiles they varied considerably in number, shape, and size.

A paper 'On the Chemical Composition of Vegetable Membrane and Fibre,' by the Rev. J. B. Reade, was then read by the Secretary. The author commenced by observing, that Professor Henslow, in his late work on Botany, had stated, that great difficulties existed in the way of obtaining an accurate analysis of the chemical composition of vegetable membrane and fibre. Having observed the accuracy with which his friend, Mr. Rigg, of Walworth, analyzed vegetable products, he recommended him to commence a series of experiments on this subject, and obtained the following results:—

Spiral vessels from the Hyacinth yielded—

Carbon	41.8
Hydrogen	1.1
Nitrogen	4.3
Water	51.8
Residuary matter	1.0
	100.0

Cellular tissue:—

Carbon	39.2
Oxygen	7.4
Nitrogen	3.9
Water	48.5
Residue	1.0
	100.0

An analysis of different parts of the flower-stalk of the Hyacinth gave the following results:—

	C.	H.	O.	N.	W.	Res.
Epidermis and stomates	41.7	—	2.0	4.0	50.8	1.5
Cellular tissue beneath epidermis	41.8	—	2.1	4.1	50.5	1.5
Woody fibre under bark	39.2	0.5	—	5.7	55.6	1.0
Spiral vessels	35.8	1.7	—	3.9	58.1	0.5

In these experiments, the existence of nitrogen to so great an extent was pointed out as remarkable. The author also thought they tended to prove that vegetable fibre was not a form of membrane, as generally supposed.

Professor Henslow observed, that, in his work, he had alluded to the great difficulty of isolating entirely either fibre or membrane. The cells of the cellular tissue must contain some fluid matter in their interior, besides the fibre that lined them externally. Mr. Rigg had experimented on spiral vessels which contained both membrane and fibre; therefore, the ultimate composition of membrane and fibre was still a desideratum.—Professor Lindley said, it was important that facts of this kind should be well made out. As a proof of want of care in the paper, it might be inferred, from the author's statements, that there were no spiral vessels in the petals of the hyacinth; but the fact is, they are very abundant. It appeared to him, that the author had mistaken cellular tissue and woody fibre, for the elementary membrane and fibre, the chemical analysis of which was so difficult.

Rev. J. W. Hope read some 'Remarks on Filariæ, a Genus of Parasite Worms, recorded as infesting Man and Insects.' His object, he stated, was to call the attention of the Section to this class of parasites, and to solicit authentic information respecting them. He believed they first attacked insects in their larval state, and grew with their growth. He thought one of the uses of parasites might be to control the exuberance of species. He presented to the meeting a table of 40 genera, and several species of insects, infested by Filariæ. To confirm the opinion of these parasites attacking the larvæ of insects, he instanced those genera, as Acilius, Colymbetes, and Phryganea, in which the larvæ are aquatic. Phryganiæ are frequently the subjects of these attacks, which could not well take place after they had emerged from the pupa state. He had not, however, succeeded in detecting Filariæ in larvæ of any kind. Rudolphi had stated, that he thought all the parisitic Filariæ were of the same genus: but he doubted this; and even Rudolphi had marked one as "genus doubtful." The Filariæ found in Phryganidæ differed from those of Coleoptera. The species of the genera Ascaris and Filaria, he thought would bear distributing into several other genera. The species found in the Phryganidæ appears intermediate between Gordius and Filaria. The author concluded by suggesting, that the term Filaria be restricted to the Filaria Medinensis (or Guinea-worm), and its congeners; while several other sub-genera may be formed, to include the parasites infesting insects.

Mr. Duncan inquired, if it was the opinion of Mr. Hope that each genus of insects was attacked by its peculiar parasites. It had been stated, at a former meeting of the Association, that this might be one means of determining the genus or species of animals; and, if the parasites were constant, it would undoubtedly be a valuable means of diagnosis.—The President observed, that he had seen parasitic worms in the Arachnida; and in the *Bibliothèque Universelle*, there was an account of a Filaria found on a species of Gryllus. The economy of these animals is very curious: they may be dried, and brought to life again by moisture at an indefinite period. They are common in the waters of clay soils, and may probably be introduced into the system in the same manner as the Fasciola, which produces the "rot" in sheep, and which exists in the water from which the animals drink.—Mr. Hope replied, that he believed each genus of insects had its distinct parasite, and he thought that even now he could tell to what order an insect belonged by examining its parasitic invaders.

Mr. Bowman read a paper from Mr. Gardner, 'On the Internal Structure of the Wood of Palms.' The attention of Mr. Gardner, who is residing in Brazil, was directed to this subject by the remarks made by Professor Lindley, in his 'Introduction to Botany.' In order to test the truth of the theory of Mohl, he made several experiments on the palms in his district. He made a vertical section of a palm four inches in circumference, and, by doing this, he could trace very plainly woody fibres proceeding from the base of the leaves to the centre of the stem, at an angle of 18 degrees; they then turned downwards and outwards to within a few lines of the external cortical part of the stem, running parallel with its axis. The distance between these two points was about two feet and a half. The fibres were traced quite distinctly up into the centre of the leaf. In answer to the questions proposed by Lindley, in his work, the author stated:—1. That the wood of palms was always hard and compact outside, gradually getting softer towards the centre, the fibres of the upper leaves not descending to so great a depth as the lower. 2. The wood is much harder at the bottom than any other part of the stem, the inhabitants of tropical climates using only this part for economical purposes.

Professor Lindley observed, that this paper confirmed the views of the structure both of endogens and exogens, which had been increasingly embraced by botanists. In the first place, the views of Mohl on the structure of endogens were confirmed. There was, however, a slight difference between Mr. Gardner and Professor Mohl; the latter having stated that the woody fibres of endogens terminated *in* their cortical integument, whilst the former had traced them only within a few lines of this point. In the next place, the paper confirmed the theory of the formation of wood from the emanation of fibres from the leaves. Whatever might be the difference between the arrangement of the fibres of exogens and endogens, there could be no doubt that their origin was the same. Mr. Gardner had referred, in his paper, to the glandular disks on the woody fibre that were, at one time, thought to characterize the order Coniferæ. He would, however, draw the attention of the Section to a fact that had lately been discovered, and not hitherto published, that these glandular disks existed on all the woody fibres of plants that yielded resinous matter. Brown first discovered them in the wood of Tasmania (*Winteraceæ*), and Griffiths had since demonstrated them in Spherostema (*Schizandreæ*).

Mr. Nevan detailed some experiments on Vegetable Physiology. The experiments were performed on elms, forty years of age, in February 1836.

1. The stem of the tree was denuded, in a circle, of its cortical integument alone, leaving the alburnum beneath uninjured. On the May following the denuded part was filled up by the exudation of bark and wood from the upper surface of the wound, and the tree had not suffered in growth.

2. The bark and *cambium* were removed in the same manner. In August 1837, this tree sickened, and there was no formation of wood or bark in the wounded part. Two developements, however, took place, one above the other, from below; the former having the appearance of roots, the latter were branches with leaves.

3. The bark and two layers of alburnum were cut away. The tree was at the time unhealthy; it, however, put forth its leaves on that and the ensuing spring, but shortly after died. No sap was observed above or below the wounded part. Roots were developed from the upper, and branches from the lower part of the section.

4. The bark and six layers of alburnum were taken off. The tree became much less vigorous, but did not die, and otherwise presented the same appearance as the last.

5. The bark and twelve layers of alburnum were stripped. The consequences were again similar to the last two; the alburnum above and below the cut being dry, but an accidental cut that penetrated into the heart-wood exuded sap.

6. This was a repetition of the experiment of Palisot de Beauvais, by cutting away a circular ring of bark around a single branch. The branch continued to grow, and roots sprouted from the under surface of the isolated bark and branch.

7. In this the whole of the wood of the tree was cut away, except four pillars, composed of bark and sap-wood. In this case, the sap first appeared from above, descending by the pith, and then from the heart-wood the alburnum being dry. In this case

the sap must have passed up the alburnum, and horizontally through to the heart-wood.

Mr. Nevan inferred from these experiments—1. That the life of the tree does not depend on the liber or cambium. 2. A descent of sap takes place before the developement of leaves. 3. That new matter arises from below; which had not previously been allowed. He thought there were two distinct principles in the tree,—one, the ascending, or leaf principle; the other, the descending, or root principle. Mr. Nevan had also performed some experiments on the conversion of roots into branches, and came to the conclusion, that buds or branches might be developed from any part of the root above its extreme end, from which point it was impossible for buds to be developed.

Professor Lindley remarked that these experiments confirmed entirely the theory of the structure of wood adopted by Du Petit Thouars. He did not think that the existence of any new principle could be inferred from the experiments. In the seventh experiment, the horizontal circulation of the sap was proved, and confirmed the accuracy of Hall's experiment of cutting a tree nearly through on alternate sides, when the sap still ascended.

Mr. Gray then made some remarks on some rare Mammalia in the Liverpool Philosophical Museum. 1. A new species of otter from Brazil, intermediate between the genera Lutra and Enhydra. It possessed broad flat feet and tail, and flat head, but not so marked as in the marine otter, and much more so than the land otter. 2. A young specimen of the hyæna of New Holland (*Thylacinus cynocephalus*), perhaps the only one in Great Britain. 3. Two specimens of the Antelope Philantomba, which had only been described from a young specimen in the British Museum, and on this account the size had been unknown. The present full-grown specimens settled this point. 4. A rare animal, only known in this country through a bad specimen brought from Java, named *Felis Javanensis*. 5. *Phoca Lemura*, 12 feet long, being a very fine specimen, and probably one of the largest seals in a preserved state in this country.—Mr. Gray observed that all these animals were acquisitions to science, and would be valued as such by the zoologists present. He also observed on the inducement it offered for further exertions on the part of local museums.

Professor Lindley read a paper on the plant, drawings of which were presented yesterday.* It had been sent him to describe by the Geographical Society. It did not belong to the genus Nymphæa as at first supposed, and he therefore proposed to call it *Victoria regalis*. He then read Mr. Schomburgck's account of its discovery, and pointed out its difference from Nymphæa. He hoped that the opportunities afforded by the trade of Liverpool with America would soon be the means of introducing this truly splendid exotic to our shores.

SECTION E.—ANATOMY AND MEDICINE.

Dr. Carson in the chair.—Dr. Holland read a paper 'On the cause of Death from a blow on the Stomach, with remarks on the means best calculated to restore animation suspended by such accident.'

The writer commenced by stating, that the occurrence of death from a blow on the stomach has never received any full or satisfactory consideration. It is cursorily alluded to in treatises in which cases of sudden death, from a supposed impression made upon the nervous system, are discussed; and it is mentioned in this connexion, from being imagined to depend on the same mysterious cause. The cause of death from a blow on the stomach is referred to a shock communicated to the nervous system, by which the action of this organ is arrested. The primary impression is considered, by some, to be made upon the semilunar ganglion. The situation which this occupies, and not any peculiarly intimate connexion which it has with the heart, has suggested this explanation. Were it unequivocally shown that the heart derives its contractile power from this ganglion, and that this is injured, or in any way affected by the blow, cause and effect would be too indissolubly united to admit of dispute. No one has, however, shown that the heart receives its nervous energy from such source, nor are there any facts demonstrating that this ganglion is injured, or its functions disordered by a blow. No distinct evidence is, indeed, presented, proving that this occurs; writers on this subject speak only of suspended or deranged nervous action, and the effects of a shock on the nervous system.

* See Report of Botanical Society in Athenæum, p. 661, and ante, p. 671.

The circumstances which have led to the adoption of the prevailing notion may, perhaps, be reduced to three:—1. Situation of the ganglion. 2. The spot where the blow is received. 3. And the consequent fatal effect. These circumstances are the only reasons adduced; and yet, without other corroborating facts, they are scarcely deserving of notice. If the plexus or semilunar ganglion be considered as a centre of nervous energy, this does not supply the heart or chest generally, but, indeed, the aorta and abdominal viscera; and hence a blow on the pit of the stomach, were its effects transmitted directly through the nervous system to the organ supplied by it, would be more likely to disturb the functions of these viscera than the action of the heart. It is not unphilosophical to contend, that an injury inflicted on this centre will disturb the organs dependent upon it; but the heart receives nervous influence from the brain, spinal cord, and sympathetic nerve, previous to the formation of the lunar plexus, or ganglion; and, therefore, if affected through either, it cannot be explained to depend on the deprivation of nervous fluid, but on the transmission of an impression or undulation.

In entering upon this inquiry, the first step was to determine the important organs peculiarly liable, from their situation and functions, to be deranged by a blow on the stomach. Those were the aorta and vena cava ascendens. The pit of the stomach is unquestionably the situation where these large and important vessels are alone liable to severe functional derangement from a blow. Above this point they are securely protected by the parietes of the chest, and below it by the mass of abdominal viscera. A blow in this situation has necessarily a tendency, whether it strike the artery or the vein, to urge the circulating fluid towards the heart. Nature, by means of the semilunar valves, has prevented the frequent occurrence of such an accident; but the violence of the blow is quite sufficient to overcome this obstacle or barrier to the retrograde motion of the blood. The fatal result is to be referred to the sudden propulsion of arterial fluid into the left ventricle, and not to the greater force with which the venous blood may possibly be returned to its destination. In discussing this subject, there are three points to which especial attention should be given. 1st. Is the aorta so situated as likely to be influenced in the manner stated? 2ndly. Would a blow, given with great violence, cause a retrograde motion of blood, and its entrance into the left ventricle? 3rdly. Would the latter cicumstance be sufficient to cause death?

The latter part of the paper was occupied in endeavouring to establish the principles laid down; showing, that death from a blow on the stomach is not, as has always been considered, referable to any injury or impression made on the nervous system.

Dr. Copland required an explanation of three questions. Had Dr. Holland observed the morbid appearances after death from blows of the stomach? 2ndly, Is it possible to produce the retrograde motion of the blood from the part of the artery impinged to the heart without rupturing the semilunar valves? 3rd, Had Dr. Holland much experience of the treatment?

Dr. Holland knew that his views admitted of discussion and of objection, since they differed so widely from the received notions. He had only observed one case, and it was fatal. The post mortem examination showed a florid appearance of the mucous membrane of the stomach, especially at the pit, not unlike what is observed during digestion. The blood was fluid. With regard to the rupture of the valves, he could present no facts; but, in his opinion, the valves would not be seriously injured, and failure in resuscitation could not be attributable to the state of the valves.—Dr. Copland had not himself witnessed the morbid appearances of sudden death from a blow on the stomach, but several such were on record, which were sufficient to show the influence of the ganglionic nerves. He thought retrogradation of the blood's current, as mentioned by Dr. Holland, could not take place without a rupture of the valves, or even of the heart itself. The heart could be injured in these cases physically, from the diaphragm as well as the vessels; but, in considering this kind of death, we are not to attribute its cause to any part of the body singly, but to take a comprehensive view, and to take in the whole collectively—the respiration—circulation—epigastrium—ciliary plexuses of nerves—though one might be more injured than another.—Dr. Johnson had had his attention directed to this subject from an early age. At school there was a boy a great pugilist, who, in his rencontres, gave what he called his heart-ing, meaning a blow over the region of the heart. The first, and principal effects, were a sudden suspension of respiration, which was relieved and followed by gasping; the heart, oppressed, did not cease its action, for, if it did, deliquium animi would ensue: the pain was very peculiar. He agreed with Dr. Copland as to the cause of death in these cases, and could not conceive that a forcible impression of the aorta could throw the blood back, and force the valves; for in dead bodies this was difficult, though the valves were then flaccid, and here it was said to occur to the living—being still more difficult.—Sir James Murray had seen two cases of suspended animation from blows on the stomach; one recovered, and the other died. The remedy he should recommend, would be to throw a bucket of cold water over the body—gasping would ensue, and respiration follow. He could not agree with Dr. Holland, more especially when he considered the slight influence the small quantity of forced blood would exert. A space of the artery, equal at most to about two inches, would be struck—the blood contained in one inch would be distributed downwards through the iliac arteries to the extremities, where space enough was found; and the other inch of blood sent upwards, would find a much greater space through all the vessels of the chest and upper extremities. He would not deny that an inch of blood above the healthy quantity, suddenly propelled into the heart might injure that organ, but thought there was not enough to injure the heart and vessels, as they must be, from sudden violent blows on the stomach.—Dr. Williams had not read any author who gave a satisfactory account of the effects of blows on the stomach, but he agreed in opinion with Dr. Copland. He had paid particular attention to the semilunar valves, and must differ from Dr. Johnson. They are perfectly mechanical, and act in the dead as in the living bodies. They completely stop all entrance to the heart, and must be ruptured for entrance there. Dr. Williams then alluded to death from syncope, by poisons, crushing of the limb, ingestion of cold water,—and concluded by recommending treatment by diffusible stimuli.

Dr. John Reid then gave an account of an experimental investigation into the Glosso-pharyngeal, Pneumogastric, and Spinal accessory Nerves. This communication was stated by Dr. Reid to contain merely a short epitome of some lengthened remarks, which he had drawn up on this subject, but it embraced the principal results, deduced from the numerous experiments which he had performed upon those complicated and important nerves, generally included under the eighth pair.

Glosso-pharyngeal Nerve.—The experiments on this nerve were all performed on dogs, and were twenty-seven in number. Seventeen of these were for the purpose of ascertaining if it was to be considered a nerve both of sensation and motion, and what were the effects of its section upon the associated movements of deglutition, and on the sense of taste. The other ten were performed on animals immediately after they had been deprived of sensation, with the view of ascertaining more accurately how far it is to be considered a motor nerve. The phænomena observed in these experiments were first stated; and the conclusions drawn from a review of the whole of the data thus obtained, were these:—first, that this is a nerve of common sensation; second, that mechanical and chemical irritation of this nerve before it has given off its pharyngeal branches, or of any of those branches individually, is followed by extensive muscular movements of the throat and lower part of the face; third, that the muscular movements thus excited, depend, not upon any influence extending along the branches of the nerve to the muscles moved, but upon a reflex action, transmitted through the central organs of the nervous system; fourth, that these pharyngeal branches of the glosso-pharyngeal

nerve possess endowments connected with the peculiar sensations of the mucous membrane, upon which they are distributed, though he cannot pretend to say positively in what these consist; fifth, that this cannot be the sole nerve upon which all these sensations depend, since the perfect division of the trunk on both sides, with removal of a considerable part of it (if care be taken to exclude the pharyngeal branch of the par vagum, which lies in close contact with it), does not interfere with the perfect performance of the function of deglutition; sixth, that mechanical or chemical irritation of the nerve, immediately after an animal has been killed, is not followed by any muscular movements, provided care be taken to insulate it from the pharyngeal branch of the par vagum; seventh, that the sense of taste is sufficiently acute after the perfect section of the nerve on both sides, to enable the animal readily to recognize bitter substances; eighth, that it may probably participate with other nerves in the performance of the function of the sense of taste, but it certainly is not the special nerve of that sense; ninth, that the sense of thirst does not depend entirely upon this nerve.

Pneumogastric or Par Vagum Nerve.—From the results of thirty experiments on this nerve, he is satisfied that severe indications of suffering are generally induced by pinching, cutting, or even stretching this nerve. Powerful respiratory movements were excited in some of the animals, when the trunk of the nerve was compressed for a few moments by the forceps.

Pharyngeal Branches of the Par Vagum.—From seventeen experiments performed on dogs, either when alive, or immediately after being deprived of sensation, he concludes, that these are the sole motor nerves of the constrictors of the pharynx, the stylo-pharyngeal and palatine muscles; and that the sensitive filaments contained in these branches of the par vagum are exceedingly few, if under ordinary circumstances they exist at all.

Pharyngeal Branches of the Par Vagum.—From his experiments on these nerves, repeated and confirmed in various ways, he concludes, that all the muscles which move the arytenoid cartilages, receive their motor filaments from the inferior laryngeal or recurrent nerves. That one only of the intrinsic muscles of the larynx receives its motor filaments from the superior laryngeal, viz. the crico-thyroid muscle, and that consequently, the only change which this nerve can produce upon the larynx as a motor nerve is, that of approximating the cricoid cartilage to the thyroid,—in other words, of shortening the larynx; and that the sensations of the larynx depend upon the superior laryngeal nerve. These experiments are completely subversive of the statement of Majendie, that the inferior laryngeal nerves supply those muscles only which open the glottis, while the superior supply those muscles which shorten the glottis. They also illustrate in a very satisfactory manner the causes of the dyspnœa, amounting in some cases to strangulation, when the inferior laryngeal nerves are cut or compressed.

He has also satisfied himself, that when any irritation is applied to the mucous membrane of the larynx in the natural state, this does not excite the contractions of these muscles, by acting directly upon them through the mucous membrane, but that this contraction takes place by a reflex action, in the performance of which the superior laryngeal nerve is the sensitive, and the inferior laryngeal is the motor nerve. He is also convinced that the muscular contractions of the œsophagus, are not called into action by the ingesta, acting directly as an excitant upon the muscular fibre, through the mucous membrane, but by a reflex action, part of the œsophageal filaments acting as sensitive, and others as motor nerves. Our space will not permit us to state any of the results obtained from the experiments on the other branches of the par vagum.

Spinal Accessory.—This nerve was cut across in seven dogs at its exit from the cranium, and no effect upon the voluntary movements of the muscles of the neck could be observed. In other animals the nerve was first cut across on one side, and then a weak dose of prussic acid given. This was frequently followed by powerful, slow and regular respiratory movements, during some of which distinct contractions of the sternomastoid muscle were observed in unison with the other muscles of inspiration.

The next paper was entitled 'Observations on the Structure of the Sacrum in Man and some of the lower classes of Animals,' by Hugh Carlisle, M.B.—Mr. Carlisle exhibited to the Section several anatomical preparations of the human sacrum in different states of growth, in which the separate formation of the lateral parts, consisting both of ribs and of transverse processes, was distinctly shown. The analogous structure in certain classes of reptiles were displayed by means of preparations and original drawings, and the errors of descriptive anatomists on these subjects were pointed out. Mr. Carlisle showed that some of the saurian reptiles afford the best examples of distinct and well-developed sacral ribs, although this peculiarity in their structure has been wholly overlooked by previous anatomists. In these animals the sacral ribs are two in number on each side, the anterior being articulated to the bodies of the last dorsal and the first sacral vertebræ, and to the inter-vertebral fibro-cartilage, the posterior to the last sacral and the first caudal vertebræ. In the human subject the sacral ribs are four on each side, and they remain in a separate and distinguishable state until the age of from three to seven years, after which period they are all, except in rare instances, consolidated with each other and with the bodies and transverse processes of the sacral vertebræ. The os ilium in the fœtal state, and for some years after birth, in the human subject, is connected to only the first two at each side of the sacral ribs, a fact which is consistent with the imperfect developement, at that period, of the lower extremities, and with the disposition, at an early age, to walk on all fours, and which affords an additional example to those already known of the similarity which prevails between the temporary forms of certain parts of the human body and the permanent dispositions of the corresponding parts in animals of a lower class. In many of the quadrumana, of quadrupeds, and of reptiles, two is the number of sacral ribs constantly in apposition with the os ilium: in the human subject, at a more advanced period of growth, the os ilium at each side is connected by a cartilaginous intermedium to the extremities of three sacral ribs, and, in one instance, in the skeleton of a negro, Mr. Carlisle observed it conjoined to four. This communication was concluded by some observations on the peculiarities of structure exhibited in the skeletons of the Testudo Græca and the Testudo Midas, the former of which Mr. Carlisle considers to possess *two* sacra, one for the posterior and one for the anterior extremities; while the latter has but one sacrum belonging to the posterior extremities, and, possessing a more extensive range of motion in the anterior, presents in them nearly the same mode of junction to the spine which prevails in birds and some quadrupeds.

A paper was then read by Dr. Black, on the Epidemic Influenza which occurred this spring at Bolton; but as it contained some important tables, requiring great care to ensure accuracy, we defer our report until next week.

Section G.—MECHANICAL SCIENCE.

The President having taken the chair, Mr. J. I. Hawkins laid before the meeting as a specimen of mechanical sculpture, a marble bust of about six inches in height. At the meeting in Dublin, busts and statues were exhibited; this however was the first perfect specimen *wholly* finished by the machine. The machine itself he described as a species of engine lathe, in which the bust to be copied, and the block of marble to be sculptured, are placed in a frame, capable of almost universal motion; to that the block to be cut, may be applied in all directions to a cutter in the lathe, while all the parts of the model are brought necessarily in contact with an index fixed at such a distance from the cutter, as are the corresponding parts of the model and of the block. Some cutters, he said, were as small as a pin's head, and in many parts of the machinery an error of 1000th part of an inch would be destructive. Mr. Hawkins stated that he and Mr. James Watt had both commenced their experiments in 1804,—Mr. Watt having applied himself to medallions, Mr. Hawkins to round figures.

Mr. Fairburn then read a Report on the comparative strength and other properties of Cast Iron, manufactured by the hot and cold blast.

At a previous meeting of the Association, Mr. Hodgkinson read a Report on the comparative strength and other properties of iron manufactured by the hot and cold blast.—In the prosecution of inquiries since made, it was conceived desirable to subject the metals operated upon to more than one species of strain; to vary their forms, and, by a series of changes, to elicit their peculiar, as well as comparative properties. First, they have been drawn asunder by direct tension. Secondly, they have been crushed by direct compression both in short and long specimens, (the results of which will be given in a paper read on Thursday); and, Thirdly, they have been subjected to fracture by transverse strain, under various forms of section, and at various temperatures. Ten bars of hot and cold blast iron were also loaded with different weights, from 112 lb. to near the breaking point, and left for many months to sustain the load, and to determine the length of time necessary to effect the fracture. The bars thus loaded, are still (with one exception,) bearing the weight, having been suspended upwards of six months, and, from what we can at present perceive, there is every chance of a long and protracted experiment. In making the experiment on transverse strain, a number of models of different sizes and forms were prepared, and the irons, both hot and cold blast, were run into the form of these models; but as there is usually a slight deviation in the size of the castings from that of the model, the dimensions of the bars were accurately measured at the place of fracture, and the results reduced, by calculation, to what they would have been if they had been cast the exact size of the model, assuming the strength of rectangular beams to be as the breadth and square of the depth, and the ultimate deflection to be inversely as the depth, the length being constant. In comparing two irons, the greatest care was taken to subject them as nearly as possible to the same treatment.

A series of experiments was also made to determine the strength of hot and cold blast iron at various temperatures, from 32° (the freezing point) to the boiling point; for this purpose, a cast-iron trough was employed, in which the bars to be broken were placed, and covered with snow or water (which was kept at the proper temperature by a jet of steam), as the case required; the weights were then gradually laid on until fracture took place.

The strength of bars made red hot was also tried, and, contrary to expectation, they retained their tenacity and power to resist the load to a considerable extent: the reduction of strength in a bar one inch square, in a range of temperature from 32° to that of redness was rather more than one-sixth, the deflection being upwards of 1½ inch in a bar 2 feet 3 inches long.

RESULTS.

Carron Iron No. 2. (Scotch).

Mean ratio of transverse strength, assuming the cold blast iron at .. .. 1,000 : 9,799
Mean ratio of power to resist impact .. 1,000 : 1,036.9

Whence, in the transverse strength of Carron iron No. 2, using a variety of forms of section, the strength of the cold blast is to that of the hot blast, as 100 to 98, nearly.

Devon Iron, No. 3.

Mean ratio of strength in sections of various forms (thirteen experiments) .. .. 1000 : 1409
Power to sustain impact .. .. .. 1000 : 2742

This is an exceedingly hard iron, with a singular appearance, the centre or more granulated parts of the fracture being surrounded with a circle having the appearance of hardened steel.

Buffery, No. 1, Staffordshire iron cold and hot blast.

Mean ratio of breaking weight .. .. 1000 : 925
Mean ratio of power to resist impact .. 1000 : 965

In the buffery iron, the hot blast manufacture is weaker whether we view it in its transverse strength, or its power to resist impact.

Coed Talon, No. 2, North Welsh iron.

Mean ratio of strength in a number of experiments .. .. 1000 : 1014
Mean ratio of power to resist impact .. 1000 : 1219

Modulus of elasticity in lbs. for a bar of one inch square.

$$\text{Cold blast} \left\{ \begin{matrix} 14{,}680{,}000 \\ 13{,}947{,}000 \end{matrix} \right\} 14{,}313{,}500 \text{ lb.}$$

$$\text{Hot blast} \left\{ \begin{matrix} 15{,}810{,}000 \\ 12{,}835{,}000 \end{matrix} \right\} 14{,}322{,}500 \text{ lb.}$$

Elsicar Cold Blast, No. 1, against Melton Hot Blast No. 1, (Yorkshire Iron.)

Mean ratio of strength .. .. 1000 : [illegible]
Mean ratio of power to resist impact .. 1000 : [illegible]

The modulus of elasticity in all the irons are computed; but only given in a few cases in the results.

Relative strength of hot and cold blast iron to resist a transverse strain at different degrees of temperature.

Cold blast 949.6 at 32°. Hot ditto 919.7, Mean.
Ratio of strength, 1,000 : 977.6.
Power to resist impact, 1,000 : 1,039.
Cold blast 748.1 at 191°. Hot ditto 823.6.

In these experiments, it appeared, that the cold blast lost in strength from 32° up to a blood-red, perceptible in the dark as 949.6 to 723.1; whereas, in the hot blast the strength is not so much impaired, being as 917.7 at the freezing point, and 829.7 when perceptibly red in the dark.

In all former experiments on the transverse strain of cast iron, it has been assumed, that the elasticity remained perfect up to one-third the breaking weight. In pursuing these experiments, discrepancies were noticed, and results widely different to those generally received were observed. It was found that one-seventh, and, in some cases, one-eighth the breaking weight was sufficient to produce a permanent set. These facts induced an extended series of experiments, principally to determine what load was necessary to effect a permanent set; and, if such weight, continued for an indefinite time, would break the bar. It became a question of great importance to know, if a weight, having once impaired the elasticity, would or would not, if continued, increase the deflection. The inquiry, therefore, was—To what extent can cast iron be loaded without endangering its security? To solve this question ten bars of hot and cold blast, differently loaded, were placed upon a frame, to ascertain the amount of deflection at stated periods, and to determine what was necessary to break the bars with their respective loads.

	Inches.
In the cold blast, with a load of 280lb., the deflection increased in 103 days from	1.025 to 1.033
Hot blast, ditto from	1.173 to 1.197
Cold blast, with a load of 336lb., increased in 105 days, from	1.344 to 1.366
Hot, ditto, from	1.573 to 1.627
Cold, with a load of 392lb., increased the deflection in 108 days, from	1.786 to 1.843
Hot, ditto, from	1.891 to 1.966

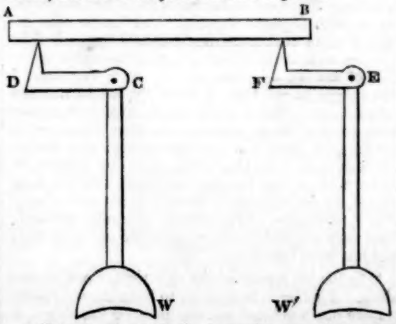
Cold blast, with a load of 448lb., continued to increase in deflection, and ultimately broke, after sustaining the weight 35 days. All the bars from the hot blast broke in the act of loading them with the above weight, 448lb.

Mr. Fairburn stated, that all the irons were made of the same materials, and under the same circumstances. The irons were of fifty sorts.

Mr. Cottam inquired as to the elastic forces. Dr. Young and Mr. Tredgold had found that the strength of the material would fail if loaded beyond its elastic force: he wished to know whether the loads had been more or less than 850lb. to the foot. Mr. Fairburn stated that some of the loads were more, some less, and that a weight of 280lb. produced a permanent set of an inch square bar. The President remarked, that the calculation as to elastic forces was scarcely to be confided in. Mr. Fairburn, in answer to another question, stated, that the hot blast iron was the more flexible and better capable of bearing impact; but that all the results of impact had been taken from calculations founded on cold blast iron. Mr. Fairburn stated, that the crystalline appearance was finer in hot than in cold blast. There were no experiments made on the loss by remelting, and none on wrought iron,—all on cast iron. In reply to Mr. Cottam, he mentioned, that all the Scotch irons had no cinder; the composition of the others they did not know. Great difficulty had been experienced on this point, because the different manufacturers were unwilling to give information.—Mr. Guest professed on his part the fullest readiness.—Some conversation took place with regard to the peculiarity of appearance in the broken bars. The President remarked, that when a rectangular bar of any substance is exposed either to fracture, or even to temporary deflexion, a similar appearance was found: this was known from the experiments on glass, by polarized light. Mr. Fairburn in assent said the crystals were always more compact in the edge than the centre. Mr. Webster inquired whether the elastic weight was always less than one-third of the breaking weight. Mr. Fairburn said, always—and afterwards replied to a question from Mr. Guest, that the Scotch hot blast iron showed a greater comparative strength as compared with cold blast, but that they had made no experiments on South Welsh iron. There was a perceptible permanent set from 280lb., the experiments being of from five to ten minutes in duration, and it being possible to judge the deflexion to the 1000th part of an inch.—Mr. Webster said it had been found that the first set was owing to the breaking of the first crust, and that beyond the first permanent set up to the elastic limit, the deflexion increases exactly as the weight. Some further conversation ensued, in which Mr. Smith and others took part, when Mr. Guest suggested the propriety of further continuing these researches, to which the President agreed, and suggested a recommendation to this effect from the Committee of the Section to the General Committee. Thanks were then voted to Mr. Fairburn for the zeal and skill with which he had prosecuted these researches for the Association.

Mr. William Ettrick then proceeded to give a description of an Artificial Horizon.

He first described three methods already known, that by the peg or humming top,—the spirit level, described at the Edinburgh meeting,—and another, as given in the *Mechanic's Magazine* of July last. He then showed the imperfections to which these instruments were liable, and some methods by which they might be obviated. With regard to the humming top, he gave rules for placing the mirror at right angles to the axis of motion of the top, in which consists the great difficulty in the use of this instrument. He then, by an apparatus which he accurately described, showed the way in which the motion of the top might be continued for any length of time. Of the second method, viz. that by the spirit level, as applied to the sextants, he mentioned, that the common spirit level was inadequate to the purpose, the motion of the bubble being much too sensible for heavy seas. He had ascertained, that this motion of the bubble might be retarded in any degree required, by diminishing the internal diameter of the glass tube, the retardation being very small with a diameter of three-eighths of an inch. By decreasing the diameter to two-eighths, the retardation was very considerable, and sufficient for almost any sea. When the diameter was increased to two-tenths of an inch, the motion of the bubble was too much retarded to act as a single spirit level—thus applied to the quadrant. He then suggested an improvement by using three levels, applied parallel to each other—one being too much retarded, and another too little—the middle one possessing the perfect retardation required. By this means the bubble of the middle spirit level might be brought much more quickly to the central marks upon the tube. He exhibited to the Section an instrument of his own construction, consisting simply of a black glass plane, fixed upon a pendulous rod, supported by centres moving in gimbals. This rod had a weight attached to its lower extremity for keeping the plane horizontal. Having found that this rod acquired a pendulous motion, which lasted for a considerable length of time, he prevented this motion by allowing the rod to hang in water or oil. Finding, also, that the water itself acquired an oscillatory motion, giving a similar one to the pendulum, he decreased the area of the surface of the water as much as possible, making it only three-fourths of a circular inch, barely sufficient to allow the rod to move in it without touching the sides, the water reaching to within one-fourth of an inch of the centre of motion of the pendulum. The change of motion of the vessel being necessarily quicker than the motion of the pendulum, as retarded by the water, the plane could not in short seas come immediately to the perfect horizontal. He therefore showed a means by which this error might be corrected, which the subjoined sketch explains.



A B is the plane black glass mirror, resting upon the two points; C W and E W′ being two pendulums with the balls attached, which move upon the centres C and E. These rods have arms C D and E F applied at right angles to the axis of the pendulum rod, which must be of equal lengths, as also the pendulum rods ought to be, having perfectly equal retardations. Mr. Ettrick then explained an optical principle, by which the instruments might be still further improved. Having stated that a discovery of Lieutenant Beecher, R.N., involved part of the principle now described, he claimed for himself the priority of the invention, having tried the instrument on the 15th of October 1836, and having in his possession several letters in proof of this fact.

Professor Henry, of New Jersey College, Princeton, U.S. then addressed the Section, and said, he had been requested to present to the Association a map of the United States, in which were marked the railways and canals completed and in progress. They had been fully described in some French works lately published, and in the American Almanac. After enumerating several geographical facts well known to our readers, as to the three natural divisions of America, the Atlantic slope, the middle basin of the Mississippi, and the Pacific slope, &c. he mentioned that there were now 1,500 miles of railway in operation in the United States, and 2,000 miles of canals; and that 3,000 miles of railway were in progress, which had been in a great degree interrupted, owing to the late commercial convulsion.—In answer to a question put by Mr. De Butts, he stated, that, on the Hudson, there being very little current, 150 miles were frequently accomplished by the steam-boats in nine hours.—Dr. Lardner much doubted, whether a speed of 15 miles an hour could be generally attainable.—Mr. Webster stated, that Mr. Blunt, an American engineer, had, in a pamphlet which he quoted, declared, that the American boats had accomplished 74 miles in five hours, and that the distance from New York to Albany, 150 miles, was performed in ten hours by boats built principally with a view to speed. Dr. Lardner then proceeded to lay before the Section a part of his observations as to the practicability of applying steam to long voyages, (these will appear in Wednesday's report,) and at three o'clock the Section adjourned.

Soirée.—Town Hall, Tuesday evening.—The splendid suite of rooms at the Town Hall was thrown open on Tuesday evening for a Conversazione. This was attended by a crowd of members, visitors, &c. &c. The party, indeed, was inconveniently large. Slight refreshments were served in the course of the evening, and the meeting broke up at an early hour.

RETURN OF CAPTAIN BACK.

Captain Back's own Narrative of the Voyage.

The welcome intelligence of the safe return of Captain Back and his crew, after an absence of fifteen months, during which they were exposed to hardships and dangers almost unparalleled; and the deep interest felt by the public in the fate of this gallant officer and his intrepid crew, will cause the following authentic account of the voyage, which we have the gratification to lay before our readers, to be received with great interest.

We need hardly premise, that H.M.S. *Terror*, strengthened and prepared in every way for encountering the ice, sailed from England in June 1836, with the intention of proceeding to Repulse Bay or Wager Inlet, on the north-western shore of Hudson's Bay; thence an exploring party was to cross over the supposed isthmus to the Arctic Sea, with the hope of coasting along and determining the outline of the northern shores of America. The following letter will show that the physical obstacles which opposed themselves to this undertaking were utterly insurmountable:—

"*To the Secretary of the Royal Geographical Society.*
September 11, 1837.

"Sir,—As the expedition from which I have just returned, originated with the Geographical Society, and, at its recommendation, was most liberally carried into effect by His Majesty's Government, I feel it incumbent on me to offer to the Society an outline of the principal events which occurred from the time of my quitting England in June 1836 till my return to Lough Swilly on the night of Sunday the 3rd inst.

"In a statement of this description, it would be impossible to enter into the detail of all the extraordinary, and, I may say, unparalleled circumstances, which have marked the course of the whole of our

proceedings: such details, I trust, I may shortly be enabled to offer to the Society and to the public in a more complete form; but, in the meantime, it is due to those who took so warm an interest in the expedition, to furnish them with an authentic narrative of the voyage, which must, however, necessarily be very brief, and will consist of extracts selected from my daily journal, as better calculated to convey a correct impression of the singular occurrences to which we were witnesses.

"*June* 23.—We took our departure from Papa Westra, and steered across the Atlantic—the weather stormy. July 29.—We fell in with the ice, and on the following day we first saw the coast of Labrador, near Cape Chudleigh. Aug. 1.—Passed through Hudson's Straits, and on the 5th saw some of the Company's ships, apparently beset with ice, off the North Bluff. By keeping close in with the land we got ahead, and lost sight of them; and on the following day we were ourselves hampered. The ice was compact, and covered the horizon towards Hudson's Bay, as far as could be seen from the mast head, while to the north-west it presented a favourable appearance; I had, therefore, no hesitation in proceeding in that direction. Aug. 16.—We got a run of forty miles from Trinity Isles, yet did not get sight of Baffin Island till the 23rd, when we also saw Southampton Island to the south-west.

"Two days of westerly wind at this crisis would have enabled us to reach Repulse Bay; but easterly winds prevailed, and packed the whole body of ice in such a manner, that all hope of retracing our steps, to pass to the southward of Southampton Island, and up Sir Thomas Roe's Welcome, was out of the question.

"On the 29th, we were drifted by the ice to lat. 65° 50′ N., lon. 82° 7′ W. This was our extreme north point, and here we were within forty miles of Winter Island, where the *Hecla* and *Fury* passed the winter of 1821-2. By dint of boring, the ship was worked to the southward towards Southampton Island, whither we were attracted by the flattering appearance of lanes of open water. Sept. 4.—We were only 136 miles from Repulse Bay, and two days of strong breeze would have led through Frozen Strait to our destination. During the next fortnight we continued drifting slowly to the westward, passing within three miles of Cape Comfort—a bluff headland, rising about 1000 feet above the sea. Sept. 20.—We were seriously nipped by the ice, so much so as to start some of the ship's fastenings. On the 22nd, being within twenty-five miles of the Duke of York's Bay, we tried to cut through the ice, but found it impracticable, as it closed immediately. From this date, the ship was no longer under our own guidance, but, being closely beset, was carried to and fro according to the wind and tide. Sept. 26.—We were drifted into lat. 65° 48′, long. 83° 40′, our extreme western point, and 90 miles from Repulse Bay. Sept. 27.—A rush of ice from the eastward lifted the ship's stern seven feet and a half out of the water—constant easterly winds. Oct. 9.—A clear channel in shore as far as Cape Bylot, for the space of twelve hours, and again on the 27th; but we were so completely frozen up, we could not take advantage of it, although to effect so important an object the ice-saws, axes, and every other implement so liberally supplied by government, were put in requisition, and all the energies of both officers and crew were strained to the utmost, but in vain.

"Oct. 17.—the thermometer fell to 9° below Fahrenheit. In the beginning of November the ship was housed in, and every arrangement made for meeting the rigour of winter: snow walls were raised round the ship, and in this manner we drifted to and fro off the high land of Cape Comfort—at times carried so close to the rocks as to excite alarm for the safety of the ship.

"Dec. 21.—A furious gale from the westward drove us off shore 14 miles to the eastward of Cape Comfort, from which point the coast not before laid down in our charts was surveyed, as we drifted to the south-eastward for the distance of about 120 miles, as far as Sea-horse Point, the eastern extremity of Southampton Island. The general character of the coast—barren hills and cliffs, varying from 750 to 1000 feet above the sea.

On Christmas Day the first symptoms of scurvy showed themselves, which gradually extended itself to all hands. At one time twenty-five men were suffering severely from it, but eventually only three persons fell victims to this dreadful disease; viz.—the gunner and two seamen. In the beginning of January, during a calm, our floe of ice split with a fearful crash,—and this was the commencement of a series of shocks, that nothing but the great strength of the mass of timber and iron employed in fortifying the ship, could have withstood: as it was, the vessel strained in every direction. Feb. 18.—Early in the morning—thermometer at 33° below zero—a disruption of the ice took place, and waves of ice thirty feet high, were rolled towards the ship, which complained much—the decks were separated—the beams raised off the shelf-pieces—lashings and stones used for supporters gave way—iron bolts partially drawn—and the whole frame of the ship trembled so violently, as to throw some of the men down. Yet this was not our worst disaster. On the 15th of March, while drifting to the south-eastward, off a low point,—since appropriately named "Terror Point,"—a tremendous rush of ice, from the north-west, took the ship astern, and, although buried to the flukes of the anchor in a dock of ice, such was the pressure, that she was forced upon it, and, at the same time, thrown over to starboard—the stern-post was carried away, and the stern lifted seven feet out of the water. The same night, a second rush of ice tore up the remnants of our floe, and forced the ship on the ice, so that her forefoot was quite out of water—her sunken stern was threatened by an overhanging wave of ice, full thirty feet high, but which, providentially, stopped as it touched the quarter of the ship—the water poured in through the stern-frame—and the ship creaked and strained in every direction: provisions were got on deck, the boats lowered, and every preparation made for the worst extremity; and, in the darkness and silence of night, we calmly awaited the anticipated coming of another shock, which, to all human appearances, must have been the last.

"Heaven ordained it otherwise, and in this novel cradle of ice we were drifted, without further injury, to Seahorse Point. The ice that bore us was ascertained to be 70 feet thick, and it was not until we had sawed through long lines of 25 feet thick, at a future day, that the ship was freed from this situation. The position of Seahorse Point was determined to be 63° 43′ long. 80° 10′ west, variation 49° westerly; the lowest temperature was 53° below zero, when both mercury and brandy were frozen.

"On the 1st of May the ship, still on the ice, was drifted near Mill Island, thence to the southward of Nottingham Island, between it and Cape Wolstenholme, a perpendicular cliff of 1000 feet high, thence to the northward of Charles Island, which we reached on the 21st of June. The ice now showed symptoms of disruption, and we set all hands to work with a 35 foot ice-saw worked by shears, and on the 11th of July, having sawed to within 3 feet, the floe split in a fore and aft direction and liberated the larboard side; we immediately made sail in the ship but found we could not extricate her from an iceberg between the fore and main chains, we again had recourse to saws and purchases, when the lump of ice still fast to the ship rose to the surface of the water, and threw the vessel on her beam ends, the water rushing in with frightful rapidity. All hands were instantly set to work again, and laboured day and night unremittingly at the fatiguing but indispensable operation of sawing, till, exhausted by their exertions, I was obliged to call them in from the ice for rest and refreshment. Not a quarter of an hour had elapsed from quitting the work when a sudden disruption of the ice took place, and the mass crashed with terrific violence against the ship's side, snapping, apparently without effort, the lashings and spars that had been placed, fearing this occurrence; and, but for the merciful interposition of Providence, all would inevitably have been crushed by the mass of ice on which they had just been labouring.

"As the ice separated the ship righted and drifted along. Finding it impossible to hang the old rudder a spare one was fitted and sail made on the ship:—it was an anxious moment as we waited to see if she would answer her helm—and as she bore up before the wind, with her head towards England, a cheer of gratitude burst from all on board.

"I had cherished to the last moment the hope that the damages sustained might not be so great as to prevent my pushing for Wager Inlet by Sir Thomas Roe's Welcome, and there to beach the ship and repair damages, while some in boats carried into effect the object of our expedition; but when I found that she required two pumps constantly going to keep her free, that both outer and inner stern-posts were gone, the keel seriously damaged, besides various other casualties, I felt it became my duty, however reluctantly, to make the best of our way homewards. Fortunately, the early part of our passage across the Atlantic was favourable, but subsequently the weather became boisterous, and the ship leaks increased very much, so that we could barely keep her free with incessant pumping; to secure the ship also we were obliged to strap her together with the stream chain cable.

"On the 6th of August we again passed through Hudson's Straits, and on the 3rd of September arrived in Lough Swilly, not having let go our anchor since June 1836.

"To speculate on what might have been the result of this expedition, had I reached either Repulse Bay or Wager river, would now be idle, but I cannot resist the opportunity of recording my unaltered opinion, as to the practicability of the service, when once a party should have reached either of the before-mentioned starting places.

"The north-eastern shore of Southampton Island, has been now surveyed for the first time, by Lieut. Owen Stanley, who has also made various views of the coast, and a chart showing the track of the ship, the remarkable position in which the ship was placed among the ice, admirably illustrated by Lieut. Smyth, in a series of spirited and characteristic drawings.

"I cannot conclude this brief account without bearing testimony to the great assistance I have invariably received from Lieut. Smyth, and all the officers and crew employed under my command in this expedition, to the cheerful obedience with which all orders were obeyed, and to the steadiness of behaviour evinced in circumstances of no common trial.—I have the honour to be, &c.

"George Back."

"To Captain Washington, R.N.
Secretary, R. G. S."

OUR WEEKLY GOSSIP.

With the exception of Captain Back's most interesting letter, which could not possibly be deferred a single hour, we must this week be content to allow the assembled wisdom at Liverpool to have all the talk to itself; and, if there be a particle of liberality among the wise men, they ought to speak eloquently of our exertions. With the exception of the Mathematical and Statistical Sections (A and F), our published reports come down to Tuesday night. We could not find room for these without abridging many important papers or omitting the Secretary's Report, which, we believe, will be found exclusively in our columns. By accounts, bringing up the transactions of the meeting to Thursday night, we find that the total number of tickets at that time issued was above 1700,—that Newcastle has been fixed on as the next place of meeting,—that the Duke of Northumberland has been appointed President, and the Earl of Durham, Mr. Selby, and the Rev. W. Vernon Harcourt, Vice Presidents,—and that the Rev. G. Peacock has been appointed one of the General Secretaries, in the room of the Rev. W. Vernon Harcourt, resigned. Our Reports will, of course, be continued next week, though our paper will be less exclusively devoted to the subject; and we shall endeavour to bring up all arrears of other matters by additional double numbers. Fortunately there is little that is urgent either of promise or publication at the present moment.

TO CORRESPONDENTS.

No. 461 is now reprinted, and the Monthly Parts for August and September, 1836, containing the Report of the Proceedings of the British Association at Bristol, may still be had.

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